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PART I.

ORIGINAL COMMUNICATIONS.

ART. V.—*On the Law of Mortality in Phthisis, and the Influence of Moral Causes in producing the Disease.* By JAMES F. DUNCAN, M. D., Fellow of the College of Physicians, and Physician in Ordinary to Sir P. Dun's Hospital, &c.

ANY person who will take the trouble of reading with attention the successive Reports which have been published by the Registrar-General of England since the institution of that office, cannot fail to have two things deeply impressed upon his mind: the first is, the extreme degree of care employed in the compilation of the tables to which the Reports refer, as evidenced by the elaborate corrections introduced for the elimination of errors; and the second is, the extraordinary fact that each successive year confirms, in an astonishing manner, the results of those that preceded it.

It is scarcely necessary for me to remark the vast importance of these records, both in a national and in a scientific

point of view. To the statesman engaged in the study of political economy, they are important, because they bear directly upon all questions connected with population, the source of a nation's greatness, and the evidence of its prosperity. To the philosophical physician they are equally important, throwing a light upon disease which is nowhere else to be obtained. As official documents, bearing upon the interesting subject of statistics, they possess a value which it is impossible that any private researches, however carefully prepared, can ever obtain. In the first place, when a private individual sets about collecting information of this kind, and publishes the result to the world, we have to be satisfied of the motives which induced him to commence the undertaking before we can yield implicit credence to his statements. We naturally suspect that he has had some preconceived theory to support, and that this has, however unconsciously, coloured his conclusions. We require further to be acquainted with the opportunities he has enjoyed for collecting the information sought to be communicated, his habits of observation, and, above all, his veracity. All these are matters which it is extremely difficult to render so apparent as to overcome the scepticism that naturally attaches to conclusions drawn from such premises.

With a public officer the case is different; the nature of his appointment sufficiently accounts for his engaging in the dry details of figures, of which all statistical researches are composed; and the absence of any motive to alter the returns affords a strong probability that they are impartially recorded(*a*).

But there is another reason, in addition to the foregoing, which stamps peculiar value upon these official documents, and

(*a*) For a similar reason, the records of medical cases from private practice are always less satisfactory than those which are drawn from clinical observation in public hospitals, where a class of intelligent pupils has witnessed their progress, observed the treatment, and noted the result. The publicity of the latter is a conclusive argument in favour of the fairness of the reports subsequently presented to the world.

that is, the great extent of country over which they are spread. In all calculations of this kind it is well known that the liability to error diminishes inversely with the magnitude of the numbers treated of; in other words, the larger the field of observation, the greater the accuracy.

Now, it is obvious that it is impossible for a single individual to collect, with all the exertions he may bring to bear upon the subject, a mass of facts capable of the slightest comparison with that which have been accumulated by the Registrar-General, assisted by his numerous staff throughout the country; and it will be shown a little farther on, that, making every allowance for the veracity of private collectors, their habits of observation and accuracy, still differences will naturally appear upon the face of their registers, arising from the very circumstance of their labours being confined to limited districts, where local causes exercise peculiar disturbing influences.

I have already said that no sufficient motive, such as previous impressions, can be supposed capable of acting upon a public officer, so as to induce him to alter the returns to suit his peculiar views; in the case before us, however, such a supposition is not only improbable, but it is absolutely impossible. The extent of machinery employed in the preparation of the tables is sufficient to prevent its occurrence, because, as the gross returns consist of innumerable entries from detached localities, transmitted through various hands, it is necessary either to suppose that a concert of the most intricate kind is maintained between these subordinates and the Registrar-General himself, to produce the required result, or to admit the strict impartiality with which the whole proceeding is conducted. The difficulty of attempting to give a peculiar complexion to any part of these returns, added to the total absence of all motive for such a proceeding, renders the idea evidently absurd.

And what confirms this still more is the circumstance to

which I have previously alluded, that the returns in successive years continue to give similar results to those which were first published; a fact that strongly proves the accuracy with which they have been drawn up, and the absence of all intentional perversion in the entries. For, unless we suppose the subordinate returns entirely disregarded, and a fraudulent concoction of imaginary figures substituted in their place, it is manifestly inconceivable that a systematic design of altering, in ever so slight a degree, the documents, could be carried on year after year, and yet the entire result continue to tell the same story.

I have dwelt upon this point more than may seem necessary to many readers, because I feel that, in drawing conclusions from these tables, everything depends upon the perfect accuracy and the strict impartiality with which they have been drawn up. That errors have crept into them, notwithstanding all the care taken to prevent their occurrence, can scarcely be questioned; but they are errors arising from the imperfection of human knowledge, and not from any wilful perversion on the part of those engaged in preparing them; and what is more, these very errors, in the nature of things, tend to neutralize one another; for, if in any one instance, a case of bronchitis has been entered by mistake for consumption, there is an equal probability, the error being unintentional, that a case of consumption has been elsewhere set down for bronchitis.

The Report of the Irish Census Commissioners for 1841, with Mr. Wilde's Analysis of the Mortality Tables, is a public document similar in kind to those of the Registrar-General of England, and of equal, if not greater, importance. The high scientific character of the gentlemen employed on the work is a sufficient guarantee for the learning and ability displayed in its preparation. Having enjoyed the advantage of the previous publication of some of the English Reports, to serve as models for their guidance, they have rightly endeavoured to equal, in accuracy and in detail, their great statistical rivals. And in some respects they may be said even to have excelled them.

But in two points, on the other hand, it must be confessed, that the Irish records labour under a great disadvantage, as contrasted with the English, though the fault in no way rested with the distinguished individual who compiled them.

The first of these was the including a period of ten years in the inquiry, instead of one, as is the case in England, whereby many facts, especially in the earlier years, escaped record altogether; and the second was the absence of all provision for their subsequent repetition. Had these valuable inquiries been pursued annually from that time up to the present, during which the country has been undergoing the most afflictive dispensations of famine and pestilence, what an amount of important and authentic information would they have contained! With what confidence might our senators and statesmen have turned to their unquestionable, though unpalatable truths, and, instead of vague assertions and conflicting opinions, the naked facts would have spoken in the plain but startling language of figures, till the alarmist on the one hand, and the sceptic on the other, had learned to submit in silence to their decision.

Now, it is a curious and instructive fact, that the very same results which appear in the Registrar-General's reports, in reference to the mortality of various diseases, appear also in those drawn up by Mr. Wilde, as I shall presently take occasion to point out; with this exception, that in the latter the proportion of the whole to the population is considerably less than in the former; a fact accounted for by the defective registration as to the earlier years of the decade, by the removal of families during the interval, who alone were capable of giving the information; the voluntary mode in which the statements were furnished, and the facility with which such matters are forgotten a short time subsequent to the event. But as these causes are likely to operate with considerable uniformity, affecting equally the details of each sex, of all ages, and of every disease, it is easy to understand that the general effect of

these omissions in the register must be to leave the relative proportion pretty much the same as it would have been had no such omissions taken place.

Now the remarkable circumstance that each of these records, as they have successively appeared, exhibits a striking uniformity in the relative proportion of deaths produced by each disease, as compared with the population,—an uniformity not limited merely to the aggregate numbers, but extending also to each of the sexes, and to the different periods of life,—forces upon us the conviction that the fatal event which terminates existence does not occur, as was formerly supposed, in an irregular, uncertain manner, by a sort of accident, but is regulated by a fixed or settled principle, which may be called the "law of mortality," and which produces this unexpected harmony of results. Of course, in saying that the records of death exhibit a striking uniformity of results, I exclude from the consideration those which are produced by epidemic causes, and which are subject to periodic or peculiar variation. The former may be compared, from their stationary character, to the fixed stars in astronomy, which never appear to change their position to one another or to the spectator; while the latter resemble the planets, or, perhaps more properly, the comets of our system, and which obey a distinct law of their own.

But in using the expression "law of mortality," though not now introduced for the first time, several objections naturally suggest themselves to the mind. The first is, as to the propriety of the term. It is evidently based upon an assumed analogy between the phenomena of the material universe and of animated nature. Now there is this remarkable difference between them, that, while the facts from which the laws of matter are deduced are universal, those which we observe in living bodies exhibit numerous exceptions. For example,—in every country where the ratio of the sexes at birth has been accurately observed, it has been found that the number of male

infants exceeds, by a marked and regular amount, that of female infants. But if we limit the inquiry to a single family, we know that this proportion does not hold good.

Similar exceptions occur in all the assumed laws connected with life: is the expression then proper under the circumstances? To this objection a double reply may be given. First, that the exceptions may be apparent only, and not real; just as we know that the planets move constantly in one direction at a uniform rate, and yet to the eye of the observer they appear at times to be stationary, and at times to be moving in a direction opposite to their natural course.

The second answer is founded upon a consideration of the nature of the principle of life, which includes the idea of a separate and independent power in each individual, which enables it to resist the influence of external agencies; for example, the power of maintaining a uniform temperature in opposition to that immediately surrounding it. Now the living principle may exist in various degrees, being stronger in some instances than in others, and consequently the power of resistance may be more effectual in certain individuals than in others. In brute matter, on the contrary, no such power of resistance can be detected, but it lies completely passive under the influence of every impression to which it is exposed. Occasional exceptions, accordingly, are the natural results of the condition of existence in the one class of subjects, and not in the other. The operation of the law is secured on the average by the strength of the controlling force being made greater than the general amount of resistance to be overcome; but the vital principle being sometimes unusually energetic accounts for the occasional exceptions.

A second objection may be taken to the expression "law of mortality," from its appearing to imply the notion of *necessity*. If the divine Creator has really implanted in our nature a property of spontaneous termination by certain diseases, according to fixed general laws, it is not only improper but im-

possible to counteract His will. If such an idea be correct, the resources of medicine are valueless, and the improvement of the healing art is impracticable. But this mistake arises from proper attention not being paid to what is really implied in the notion "law of mortality," which is neither more nor less than a *tendency* to produce a certain result if uncontrolled; just as the principle of gravity is only a *tendency* in the particles of matter to approximate with an intensity varying in proportion to their masses, which may or may not take effect according as this tendency is controlled or not. When a man places a book upon a table, he does not imagine that he is profanely interfering with the laws of nature; and similarly when a physician, by the use of medicine or otherwise, attempts to stop the progress of disease, he is so far from opposing the divine will that he is actually carrying it into effect. That the powers of medicine are not worthless, and that the healing art is really capable of improvements which may have the effect of prolonging life and diminishing sickness, are facts which no candid mind can possibly deny(*a*); but the position that disease and death are the natural results of certain fixed and general laws does not in the slightest degree interfere with the admission of their truth.

Other objections may be raised to the expression on the grounds that it is inconsistent with the doctrines of the divine foreknowledge and of special providence;—but these, being theological questions rather than medical, are not suitable for discussion on the present occasion; I shall only add, that I consider them equally capable with the former of a simple and satisfactory explanation.

I come now to what is more immediately the subject of the present paper, and that is the elucidation of the law of mortality in tubercular consumption, as established by these records. In doing so I shall faithfully detail *all* the evidence they present; but it is necessary to remark that the tables are not drawn up

(*a*) See especially Combe on the Management of Infancy.

in the same way every year, so that it is impossible to continue the statistical details in the same formulary through all the Reports to the present time. This arises from the circumstance that the compilation of these supplementary documents is optional on the part of the Registrar-General, and not obligatory; and that he naturally infers, I presume, that when a certain point has been sufficiently established by a series of observations, it would be an unjustifiable expenditure of the public funds to continue the investigation further. I mention the fact to prevent the idea being entertained that I had selected a part of the materials favourable to my own views, and omitted another portion which was opposed to them. And first, as to the relative frequency of the disease in the two sexes;—it has long been a disputed question among medical writers whether men or women were more liable to consumption; some physicians maintaining one side of this question and some the other; and a few denying that any difference in this respect existed between them.

Thus we find Dr. Briquet stating that in his wards in the Hôpital Cochin in Paris he had one-tenth more male patients than female, out of a total of 109(*a*).

Professor Reid, of St. Andrew's, states, that when he was superintendent of the Royal Infirmary of Edinburgh from 1st July, 1839, to the 1st October, 1841, there were 155 male, and only 63 female patients admitted labouring under the disease(*b*).

In the Statistical and Pathological Report of the same Institution for the years 1833, 1834, 1835, 1836, and one-half of 1837, drawn up by the late Dr. John Home, it is stated that the admissions were 185 males and 112 females during that period, the accommodation provided for the two sexes being equal(*c*).

Louis, on the contrary, tells us, that he had seventy cases of consumption in women and only fifty-seven in men under his care, in wards containing forty-eight beds equally divided

(*a*) Edin. Monthly Med. Jour. 1842, p. 621. (*b*) Ibid. p. 754.
(*c*) Edin. Med. and Surg. Jour., vol. xlix. p. 6.

between the sexes(*a*); and that this fact, which leads to the idea that females are more liable to the disease than males, is confirmed by another, which exactly harmonizes with it, viz., that of forty other individuals carried off by various chronic diseases, and found in a tuberculous condition, twenty-five were females and only fifteen males. A still stronger statement in support of his opinion is quoted in the same work, on the authority of M. Benoiston de Chateauneuf(*b*), viz., that out of 43,010 patients admitted into three of the Parisian hospitals between 1821 and 1836, 1554 died of phthisis, namely, 754 men out of 26,055, and 809 women out of 16,955 ; which shows an immense excess on the part of females, when compared with the gross numbers admitted.

The third opinion is maintained by Sir James Clark in his work upon consumption and scrofula. After giving, in a tabular form, a number of statistical returns from various sources, which contradict each other in a remarkable manner, he concludes by stating his belief that, if the subject were sifted accurately, no real difference between the sexes would be detected. In drawing this conclusion, however, he has evidently fallen into the mistake of supposing that the male population living at any time exceeds the female. His words are these(*c*): "The constant equal relation of the first seven numbers is certainly most remarkable, and appears to warrant the conclusion that for every eight or nine females, ten males die phthisical, *which is very nearly the relative numbers of the sexes;* it therefore goes far to prove that the sexes are equally liable to consumption." Any one who will take the trouble of looking at any of the Census returns will easily satisfy himself that this is a mistake, and that there is always, in every settled country, a distinct excess of females over males. It is not improbable that he may have been inadvertently led into this mistake from a knowledge

(*a*) Louis on Phthisis, Sydenham edition, p. 479. (*b*) Ibid. p. 480.
(*c*) Clark on Consumption and Scrofula, ed. 1835, p. 184.

of the fact to which I have already alluded, that there is always an excess of male infants in the number actually born, and that he may have supposed that the same proportion which holds good at birth continued afterwards. But this is not the case. An increased mortality reverses the relative numbers living. M'Culloch, in his Statistics of the British Empire(*a*) attributes the preponderance of female life to the greater risks and casualties that men encounter in the naval or military service, in their peculiar employments, in their greater disposition to emigrate, &c. ; but I have elsewhere shown(*b*) that this is not the true explanation of the difference, inasmuch as *it is produced by causes operating in infancy*, and before these casualties can possibly take effect.

The following are the returns of the deaths from phthisis, in the several Reports of the Registrar-General for England:

	Males.	Females.
1837, First Report(*c*), .	12,968	14,786
1838, Second do., .	27,935	31,090
1839, Third do., .	28,106	31,453
1840, Fourth do., .	24,519	28,168
1841, Fifth do.(*d*), .	24,329	27,937
1842, Sixth do.(*d*), .	24,048	28,098

These numbers clearly prove that in England, at least, a greater number of females die of consumption annually than males; but to form a perfect comparison of the two sexes it is necessary to calculate the proportion in reference to the number of each actually living. I shall confine myself to one of the years only, and shall select 1838, because the population for the middle of that year is furnished by the Registrar-General

(*a*) Edit. 1837, vol. i. p. 410.

(*b*) Medical Statistics,—their Force and Fallacies. M'Glashan, Dublin, 1847.

(*c*) For half year only.

(*d*) The deaths in the metropolitan districts are excluded from these returns, for what reason I know not.

himself(*a*), who computes it to have been 7,668,245 males, and 7,885,615 females. This would make the proportionate mortality in that year to have amounted to 36 men and 39 women in every 10,000.

According to the Irish Census for 1841, there died of consumption in ten years, 63,635 males and 71,955 females.

I have already alluded to the circumstance that the registration of deaths in Ireland given in this Census is defective, particularly for the earlier years of the period included, owing to the mode in which the returns were collected, so that the absolute mortality appears greatly below that of the sister kingdom, while the relative proportion of the sexes is as nearly as possible the same; the ratio, calculated in a similar manner being 15·8 males to 17·3 females, in every 10,000 of the population. This result is interesting, not only as corroborating the conclusion already arrived at from the English records, but as giving countenance to the idea that any improvement in practical medicine which may have the effect of diminishing the mortality of consumption, either in the way of prophylaxis or of cure, may still operate in such a way as not to alter materially the relative liability to the complaint.

Assuming, then, the general position, that, when a whole country is made the subject of examination (for when the field is limited we shall see presently the rule does not hold), the female sex is more prone to be attacked, it becomes an interesting question to consider the circumstances that produce such a result.

So far as I am aware, only three explanations have been proposed to account for the difference. The first is that of Mr. Farr, the talented author of the tables elucidating the reports of the Registrar-General, and he attributes it to the unnatural practice of tight lacing, which prevails so commonly among females(*b*). It is scarcely necessary to say, that I fully

(*a*) Report, page 70, small ed. (*b*) Second Report, 8vo. ed, p. 73.

agree with Mr. Farr as to the injurious effects produced by the practice he alludes to, and in the opinion that "girls have no more need of artificial bones and bandages than boys;" but still I think it will appear, upon reflection, that this cause, how injurious soever it may be in other respects, can have very little effect, if any, in producing phthisis, from the fact that the disproportion between the sexes is not greater. Men never wear stays, women invariably do;—if the habit had any tendency to produce consumption, the disease should be almost unknown in the one sex, and almost universal in the other. The next explanation proposed is that which refers it to the habit of exposing the upper part of the chest, which prevails among women, but not among men. The fact that the development of tubercle first occurs in this part of the chest appears at first sight to lend some countenance to this hypothesis; but, not to mention other objections that might be raised to this view, it is sufficient to state that the argument which has been urged against the former explanation with such success, tells with equal force against this.

The third explanation refers the difference to the domestic life of females, as contrasted with the out-door employments that prevail among men. Confined to the house, either voluntarily or by necessity, the air they breathe is less pure, the play of all the organs less vigorous, the vital changes take place with less intensity, and the exhilarating influences of solar light and heat are less felt by them than by the other sex, whose occupations place them in circumstances the opposite of those just mentioned. That their condition in this respect exercises an injurious influence over them can scarcely, I think, be questioned. That it may also tend to induce the disease under consideration is extremely probable; but it must not for a moment be lost sight of, that the constitution of the female has been admirably contrived for the duties she has to fulfil; and that consequently a domestic life, with its attendant confinement, is

calculated to be less injurious to her than to man himself, whose physical development is greater, and whose habits are more active. For these reasons I am not disposed to place much reliance upon this explanation, although I admit that it is entitled to some consideration.

Before proceeding to state what is my own opinion of the cause of this difference, I think it right to call attention to a point that I believe has not been noticed by any previous writer upon this subject, but which is plainly brought out by the tables under consideration, and that is, that while females are more liable to the disease when the entire population of a country is taken into account, males, on the contrary, are more liable when the population of a city only is considered: in other words, residence in a rural district has a greater tendency to develope phthisis in females than in males; residence in a town district has a greater tendency to develope phthisis in males than in females. For the purpose of contrasting the diseases that prevail in towns and country districts, the Registrar-General has compared together the returns of several districts, partaking, as much as possible, of these two characters, and presenting in the aggregate a nearly equal population. These I shall now refer to.

	Males.	Females.
Population, Metropolis, 1837(*a*), ...	830,421	960,030
Deaths from phthisis in half-year ending 1837, ...	1369	1297
Population in five agricultural counties in 1837, ...	830,584	893,196
Deaths from phthisis in ditto, half-year ending 1837, ...	407	451
Population in provincial cities, estimated October 1, 1837(*b*), ...	838,830	923,880
Deaths registered from phthisis in ditto, in half-year, 1837, ...	1544	1496

(*a*) First Report, 8vo. edition, p. 148. (*b*) Ibid. p. 150.

	Males.	Females.
Population in counties similarly estimated, .	879,543	897,437
Deaths from phthisis in ditto, . . .	956	1,250
Estimated population in the metropolis in 1838(*a*), ...	913,077	971,767
Deaths from phthisis in ditto, 1838, . .	4,057	3,630
Estimated population of five counties in 1838, ...	843,244	899,881
Deaths from phthisis in ditto, 1838,. . .	2,744	3,061
Estimated population of provincial cities in 1838(*b*), ...	898,233	943,144
Deaths from phthisis in ditto, ...	4,319	4,339
Estimated population of counties in 1838, .	892,688	904,095
Deaths from phthisis in ditto, ...	2,977	3,756

The same thing would be equally well shown by giving the returns for Liverpool, Leeds, Manchester, and Birmingham, all of which prove that the excess of mortality is no longer on the side of females within their districts, but on the side of males. But this will be better established by the following table, extracted from the supplement to the Second Report of the English Registrar-General, p. 168, and which shows the ratio which the returns exhibit when reduced to an uniform standard of a population of 100,000:

1838.	Males.	Females.
Leeds, ...	440	477
Birmingham, ...	526	410
London, ...	451	377
Liverpool and West Derby, . . .	595	571
Manchester and Salford, ...	549	548
England and Wales, ...	378	408

Now all these towns, except the first, exhibit an excess of greater or smaller amount on the side of males in the mortality from phthisis. But it must not be forgotten that this excess, striking as it is, is yet less apparent than the reality, from the

(*a*) Second Report, p. 158. (*b*) Ibid. p. 161.

impossibility of isolating completely the two classes of population. Every town embraces within its limits a considerable suburban population, while, on the other hand, almost every rural district comprehends some town or other, which swells its mortality, and interferes with the development of the results it would otherwise produce. This plainly proves that the relative liability of the sexes is exactly reversed, according as we select a civic or a rural population for the purposes of comparison. It also upsets the last of those theories of sexes to which I have referred, put forward to account for the difference; because a residence in town, though it *alters the proportion* in the number of men and women engaged within doors, and in the open air, does not *reverse* them, as it ought to do, were it competent to explain the difficulty. This leads me to state that I believe the true answer to the inquiry will be found in a careful consideration of the difference in the moral condition of the two sexes in the circumstances supposed. In a rural population, moral influences, in which are to be included everything connected with the intellectual development, the passions and emotions of our nature, tell with the greatest effect upon women, because of their greater sensibility, and their more frequent want of occupation. But among the inhabitants of our large towns and cities, men are even more exposed than women to the play of passion, arising from over-excitement on the one hand, and the failure of work upon the other. In the retirement of the country, life is passed in a kind of gentle tranquillity, which affords sufficient employment to occupy the mind without fatiguing it, but does not call into activity the more violent principles that prevail within us, nor strain our energies to their utmost tension. The excitement of the system is not so great as what prevails in towns, but the collapse that succeeds is proportionally less also. The pleasures that are enjoyed by those who live there may not be so intense, but they are more pure and permanent.

In our great cities, on the contrary, everything is bustle and

activity. The mind is perpetually on the stretch in the acquisition of fresh knowledge, or in the pursuit of professional advancement. It is there that science makes her brightest discoveries, and trade accumulates her richest stores. It is there that ambition finds the widest field for its exertion, and vice the most favoured opportunities for its indulgence. There the love of money and the love of praise find equal scope for their development. There, the scintillations of genius fly fastest from the collision of kindred minds. There, too, the flames of passion rise highest from the angry impetus of contending interests. There, art labours with the most unsparing energy to overtake the necessities of the human family, while luxury, on the other hand, with its refinements and its rivalry, is outstepping the means of gratification within its possession. There also speculation, with its gilded bait, lures many a thoughtless adventurer on to tread the seemingly secure path to speedy independence, till at last, too late, he finds that he has been walking on the pitfalls of disappointment, and is doomed to feel in all their agony the miserable consequences of his mistake.

In a word, everything connected with the social condition of a crowded city forces upon us the conviction that moral agencies of immense influence are continually operating upon its teeming thousands, and producing the natural consequences to which they lead,—disease and death.

I have still to add other arguments in support of this view, that moral influences have a most important tendency to produce consumption; but as I have already occupied so much space in this Number of the Journal, I must reserve them for another occasion.

Art. VI.—*Report upon the recent Epidemic Fever in Ireland.*

(*Continued from page* 86, *and concluded.*)

APPENDIX TO THE FEVER REPORT ON MUNSTER.

Since the publication of the Number for February last we have received the three following communications on the subject of the late epidemic fever in Cork and its vicinity.

Dr. Callanan, has afforded us the following observations:—

"What has been called the epidemic fever of 1847 in truth commenced in this locality, and, as I am informed, in many others, early in 1846, as is abundantly proved by workhouse and other returns throughout this district. From the commencement of 1847, however, *Fate* opened her book in good earnest here, and the full tide of death flowed on everywhere around us. During the first six months of that dark period *one-third* of the daily population of our streets consisted of shadows and spectres, the impersonations of disease and famine, crowding in from the rural districts, and stalking along to the general doom—the grave—which appeared to await them but at the distance of a few steps, or a few short hours. And so in sad truth it was; for the obituary of our workhouse here, for the year 1847, gives the appalling return of 3329 deaths!! Within the month of March of that year 757 inmates of that dismal abode perished from famine and fever. Between these two fearful agents of death and desolation, it now matters little where the preponderance lay. No human foresight could have anticipated or arrested their influence, as far at least as their direct causes operated. But it perhaps remains for a wise and beneficent legislation to take a lesson from the past, and, in its mercy and consideration, to endeavour, as much as in it lies, by averting hunger and distress through the numerous and legitimate channels of creating employment for the labouring and agricultural classes, to prevent the perpetuation, and, in

future, to avert the return of that state of suffering and wretchedness which has made the very name of this unhappy country a modern proverb for poverty, disease, and humiliation, in the eyes and on the tongues of the civilized world.

"My observations must, however, be confined to its progress and character amongst the upper and middle classes, where alone my experience lay, and amongst whom, in this district, both urban and rural, it prevailed very extensively, and in numerous instances assumed a character and progress of great complexity and danger.

"In admitting this, however, I do not mean to convey the impression that the late epidemic differed in any essential or peculiar character or degree from its former visitations in this country within my memory or experience. New incursions of disease, from the alarm and excitement which they cause, however little their identity may differ in reality from their predecessors, are apt to have a darker shade of character attached to them than perhaps they would deserve by a just and dispassionate comparison. Amongst the poorer classes, the fever of 1847 admits of no question as to its severity, and its appalling mortality; but it should be recollected and admitted that this mainly arose, and was derivable from an almost unprecedented state of privation, involving of necessity, in their issue, the very first rudiments and principles of fatal and intractable disease,—hunger, cold, impure air, deficient clothing,—in short, *intense want* in its most comprehensive and extended meaning, and in its most withering effects both on mind and body.

"These causes, however, in a medical point of view, as respective of, or involving any principle of practice for our guidance, cannot and ought not to be taken for more than they are really worth. In the production of epidemic diseases they have been, and ever will be *res notissimæ*, and their effects follow as invariable sequences whenever or wherever they prevail. In this doomed country they have for years been casting, and for many more they will continue to cast, not—alas!—their

shadows, but their bitter and blighting substance, both before and after them.

" In my judgment and experience, the character and treatment of the fever of 1847 differed but little, if at all, from that of 1817, amongst the better classes. It invaded them in the rural districts in each era, in the purest localities, and quite apart from infection or contagion, traceable at least to, or derivable from any obvious influence from such an agency.

" In numerous instances, exposure to wet and cold in these periods produced the disease. Fear, and the depressing passions too,—the ' *animi pathemata*',—had a powerful and obvious influence. In the epidemic of 1817, when, after a lengthened war, peace brought a property panic, both mercantile and agricultural, the disease was easily excited, and operated most fatally; so much so, that scarcely an individual who at that period sustained, or contemplated the infliction of pecuniary losses or embarrassment, recovered from it; and, in consequence, very many of our best and most useful citizens fell victims to their fears and sensibility.

" As exemplifying fear of the disease arising from mental emotion, a very remarkable case occurred in the autumn of 1817, here. A very respectable lady, of a nervous temperament, was solicited for alms, on leaving a shop in one of our principal streets, by a female mendicant. No attention being paid to the supplicant, she dropped on her knees, and, in the spirit of a demon, invoked on the lady the visitation of the *black fever*, as she called it. The unfortunate victim of her imprecations hurried home in terror, sickened that evening, and in ten days died of malignant typhus. The catastrophe did not end here, for her husband caught the disease, and died within a few days after.

" At that period I put forward my views in a small publication, explaining the pathology which I then adopted of this disease, as well as its treatment; and, confirmed by all research or observation which has since come under my notice

in these many years of experience, I still adhere to the doctrine and opinions which I then embraced, under the full conviction that they have afforded me the most satisfactory and successful results in the management of this often mysterious and perplexing malady.

" ' When we consider the nature of those organs to which the influence of typhus fever seems especially directed, and the intimate connexion they have with vitality, we shall not be surprised at the sudden and intractable effects which are so frequently met with in this disease.

" ' In turning our attention to this subject, the brain first claims our consideration, because it is from this source that all the formidable and less manageable symptoms, which we so frequently meet with, appear to derive their origin. In unfolding the structure of the liver and lungs, we shall also find much peculiarity of organization. Now, every practitioner who is at all familiar with the progress of typhus fever must have observed the great tendency which these organs have to be affected with inflammatory and congestive appearances, under the influence of this complaint. When, therefore, we bear in mind their great vascularity, and the effects which long-continued excitement of the heart and arteries, deriving its origin in many cases from such an agent as contagion, and acting upon a delicate and complicated structure, may produce upon them, we shall not find it difficult to account for those sudden and intractable appearances which we so frequently find typhus fever assuming.

" ' When this complaint runs its course uninfluenced by any of the local derangements resulting from increased momentum of the blood, we probably shall have had nothing to contend with save a certain portion of debility, naturally arising from an exciting cause, operating for a given period of time, and modified in its nature and effects by particular circumstances, or the peculiar nature of the prevailing epidemic, &c. When, on the contrary, under the influence of great excitement in the

circulation of the blood, we find typhus assuming a more complicated character, and giving rise to many of the appearances of well-marked inflammatory diathesis, directing its influence to those organs which, from their peculiar structure, are more particularly susceptible of morbid impressions,—are we not warranted in concluding, that these effects are, in a great measure, attributable to a certain state, first, of the nervous system, and next of the circulation? If the exciting causes of typhus fever exerted their influence solely on the brain and nervous system, and produced a direct and *final* effect there, the natural consequence of such an impulse would be a state the very reverse of that which happens on the accession and during the progress of this complaint.

"'In the *synochus* of this country, which, indeed, is the more prevalent form of typhus, the symptoms of arterial excitement run so high as frequently to leave the physician doubtful of the real nature of the complaint. I have, in numerous instances, seen cases of this kind mistaken for the incipient symptoms of pneumonia, or acute rheumatism, and treated accordingly; when, to the great horror of the medical attendants, petechiæ, and other indications of typhus, soon made their appearance. These cases turned out favourably, and assumed a peculiarly mild aspect during their progress.

"'From an attentive consideration of these circumstances, we may, I apprehend, deduce the following conclusions:—first, that we cannot assign to typhus fever the character of direct debility at its onset, inasmuch as its accession is invariably marked by unequivocal indications of great vascular excitement, frequently giving rise to appearances of well-marked inflammatory affections; secondly, that the subsequent stages of debility or collapse which result from this excitement are directly attributable to it, and are very frequently proportioned to its degree of intensity; and, thirdly, that though the stage of debility that accompanies this disease should not be proportioned to the degree of previous excitement, it may be ration-

ally accounted for on a principle which all must acknowledge, though avowedly none can satisfactorily explain,—namely, the peculiar susceptibility which some constitutions evince in being more or less influenced by the same, or by different degrees of deleterious agency.

" 'It must be admitted, that virulent contagion will very frequently produce the sudden and overwhelming effects which this form of typhus assumes; and that it gives rise to these effects by its peculiar power of oppressing or destroying the energy of the nervous system,—first exciting, and then weakening the powers of the heart and arteries,—is, I think, equally rational as an assumption.

" 'Admitting, then, the question to stand on this basis, we can pretty well explain why fever arising from contagion will be more likely to assume an aggravated form than when it arises from other causes. The heart and arteries, acted upon through the medium of their nerves, the sources of their energy, and stimulated into inordinate motion, soon become weakened, and, with their corresponding veins, lose their tone; and thus begins to fail in them the power of preserving, or of restoring when disturbed, that necessary attribute of a healthy state—a just and equitable distribution of the blood. The lymphatic system, too, will not remain uninfluenced under such circumstances, but will have to encounter a share of the general disturbance, as will be evinced in many cases by increased secretion or diminished absorption'(*a*).

" The recent researches of pathologists have added to these and other views on this difficult subject, results from certain states of the blood, from which I am quite free to admit that more than surmise is fairly deducible, in settling this hitherto obscure and perplexing question. But I submit that, satisfactory *pro tanto* as these results may be, and undoubtedly are, they

(*a*) These opinions will be found stated at length in Dr. Callanan's "Practical Observations on the Pathology and Treatment of Typhus Fever," a second edition of which appeared in 1848. London, Renshaw; p. 102.

cannot absorb within themselves the entire solution of the problem, nor in any degree, perhaps, subvert the doctrine of the supremacy of nervous power and influence in their extensive agency throughout the animal economy. How, I would ask, can these changes in the nature and properties of the blood take place in this disease, by which it is so materially altered from its natural state, but through the nervous system? In my humble judgment, these useful and instructive researches give but additional force and argument to the doctrine for which I have contended more than thirty years ago.

"I think it unnecessary to go much into detail on the subject of treatment during the epidemic of 1847. Both in and out of consultation, it was my opinion that in three-fourths of the numerous cases I attended, local depletion, either from the head, the abdomen, or the chest, would have been a safe and necessary measure, especially in the early stages; in many of them, even at late periods of the disease, it was followed by obvious and most gratifying results. Cold to the head was, in most cases, essential for many days. It was attempted by many to supersede this practice by that of the early use of drastic purgatives, but I saw many instances of their injurious effects acting upon an extensive and, in this disease, for the most part, an impatient and a sensitive surface: they ran down the strength, while they left the real strongholds of the disease unreached, and, therefore, its symptoms and force unmitigated.

"In full, plethoric subjects, where no local congestion, or active determination to any great organs, existed, tartar emetic, in small continuous doses, acted most happily. Eight cases of this description, ushered in with great and apparently alarming activity of the vascular system, were kept under perfect control, for periods varying from six to twelve days, and then broke up. In four of them numerous petechiæ appeared towards the close, but no other typhoid symptoms were observable. Within my experience wine was seldom admissible until convalescence began, and even then I was very cautious

of its use. After depletion by leeches, and when the surface became cool and soft, if insomnolency or restlessness prevailed, without delirium, opiates proved, in some cases, signally beneficial. Extracts of poppy and of hyoscyamus, with a small portion of tartar emetic, gave great comfort.

"In active delirium, blisters to the stomach, the nape of the neck, and behind the ears, were always followed by beneficial results. This and free sponging of the surface with cold or tepid water very frequently, seldom failed to be attended with obvious advantage."

Dr. Cronin has furnished us with the statistical tables of the Cove Fever Hospital, which give a summary of 1239 cases, 631 males and 608 females, treated in that institution from September, 1846, to August, 1848, both inclusive, as well as the results of 200 cases in private practice. From these, taken together, it would appear that the average age of the patients was 28; the sexes being in the proportion of twenty-nine males to twenty-seven females. Of the entire number treated, seventy-five died, or 5·21 per cent., but this mortality varied from 4·50 per cent. in the cases occurring in private practice, implying those, chiefly in the middle and upper ranks of life, to 5·32 per cent. of those treated in hospital. From Dr. Cronin's carefully arranged tables it would appear that so many as ten per cent. per annum of the whole population were attacked, allowing for the ordinary increase of the people since the taking of the last census in 1841. This disparity in the returns from Cove, compared with those printed at p. 69 of the Number for February last, can only be accounted for either by the loose ideas which exist with respect to population and statistics generally, or to the calculations having been made with different materials, the mere town population having been employed in one instance, and both town and rural districts having been reckoned in the other.

Dr. Cronin says, that although an increase of fever showed itself early in 1846, it was only in January, 1847, that it exhi-

bited a rapidly extending appearance. Want of wholesome and sufficient food appeared to have influenced the disease; fever following rapidly in those persons who had suffered from privation, while wholesome and sufficient food rendered others more capable of resisting its attacks. Purpura was frequently observed long before the fever became epidemic; and amongst cattle pleuro-pneumonia prevailed extensively.

In answer to the seventeenth query,—"Whether the late epidemic differed from the former outbreaks of fever, and in what particulars?"—our informant states that it differed in the more tedious convalescence, and the greater liability to relapse; the frequent complication of dysentery or diarrhœa accompanying it or preceding its attack; and by bronchitis complicating the fever during the winter and spring months, or occurring during the course of convalescence. Recovery was frequently indicated by gradual decrease of the symptoms, or by increased action of the skin and kidneys, which latter symptom, our informant states, he was more in the habit of relying upon, and regarding as a ground for a favourable prognosis, than any other. The average duration of the fever was about twelve days; and convalescence was, in general, very slow. The usual sequelæ of the fever were erysipelas of the head and face, dysentery, and anasarca in a great many instances; and in one case total disorganization of the eye, from ulcerative inflammation, ensued. The treatment consisted in alterative mercurials and diaphoretics; some cases required wine, and some few local bleeding; mercurial alteratives were employed in all cases, and in some carried to slight ptyalism, which was found to be decidedly advantageous; and in all cases complicated with irritation of the digestive tube, opium, in various combinations, was given with marked benefit.

Report of fever cases treated in the North Infirmary, Cork, during the epidemic of 1846, by John Popham, M. D.:

"The state of health in this city was not below the average during the early part of 1846. Fever of a gastric type was

rather prevalent in May, and cases of dysentery and scurvy were observed, the latter sometimes consecutive upon an attack of fever, but no serious amount of illness existed before the failure of the potato crop. The pressure from without upon the city began to be felt in October, and in November and December the influx of paupers from all parts of this vast county was so overwhelming, that, to prevent them from dying in the streets, the doors of the workhouse were thrown open, and in one week 500 persons were admitted, without any provision, either of space or clothing, to meet so fearful an emergency. All these were suffering from famine, and most of them from malignant dysentery or fever. The fever was, in the first instance, undoubtedly confined to persons badly fed or crowded into unwholesome habitations; and as it originated with the vast migratory hordes of labourers and their families congregated upon the public roads, it was commonly termed "the road fever." At the outset it was kept in abeyance by the ordinary means employed in this city for fever cases; but the Cork Fever Hospital was obliged, in January, 1847, from the increase of fever in the city, to refuse the workhouse cases. After a delay of some weeks in endeavouring to set to work the complicated machinery of the Board of Health, the local authorities resolved to act for themselves, and, on application, the trustees of the North Infirmary gave up their hospital, which was, I believe, the first public step made in this county for meeting the epidemic. The hospital was ready for the reception of patients on the 23rd of February, and was worked by the medical staff(*a*) for three weeks before the Board of Health was ready to give the necessary sanction.

"The chief haunts of fever in the city all through the epidemic were the cheap lodging-houses in the suburbs, which were always densely crowded by paupers flocking to the city, chiefly from the western district, *en route* for England. From

(*a*) Drs. Howe, Finn, and Popham.

these nursing places of contagion it quickly extended itself to the dwellings of the working classes, and, at a later period, it spread among the wealthier inhabitants. It was hard for the latter to escape, as crowds of importunate mendicants were allowed to beset not only the streets but the shops and doors, so that in passing through, these persons were at once sickened by the noisome effluvium, and several took the disease.

"The number of patients admitted into the Infirmary, from 23rd February to 6th September, 1847, amounted to 1602. Of these 897 were males and 705 females. We found that no age enjoyed an immunity from the disease, as we had patients from the extreme limits of human life, namely, from four days to seventy-eight years. The age most frequently attacked was from 15 to 20, the cases at this time of life being 334, over one-fifth of the whole. The next age, in order of frequency, was from 20 to 25; of these 244 persons were admitted. The periods from 10 to 15 and from 25 to 30 were about equal, the numbers being 228 and 227. In the decennial periods from 30 to 40 we had 258, and from 40 to 50 there were 147. From birth to ten years 97 cases were brought to hospital, and from fifty years upwards there were 89. The epidemic appeared to reach its culminating point in this city in the month of July, beginning to decline in August, when, in the course of a fortnight, the type was suddenly converted to dysentery. The North Infirmary was soon insufficient for the numbers hourly applying, and in March and April two other large hospitals were opened, and sheds added to the Cork Fever Hospital, so that in a population of 80,000 we had for several months from 700 to 800 patients under daily treatment for fever.

"Without giving any opinion upon the communicable nature of the epidemic, I shall merely observe that we were compelled to act on the supposition that it was contagious. Thus when the disease appeared in a family it spread rapidly, unless immediate separation was enforced and sanitary precautions at once adopted. In the hospitals almost all the nurses and

laundresses were attacked, and, instead of becoming habituated to the disease, and thereby rendered fever proof, they were sooner or later stricken with the epidemic, and generally in its severest type. Again, it was found imperatively necessary to separate the convalescent patients for two or three weeks, as their too quick return to their families contributed to spread the disease. A curious example of the communicable nature of the ailment occurred in the board-room of the Cork workhouse. Three of the guardians were stricken with fever on the same day, and it was afterwards observed that they happened to sit at a particular side of the room, in a draught of air which, perhaps, conveyed the concentrated effluvia from a crowd of paupers waiting in the confined ante-chamber for admission. No case occurred in any other part of the room.

" In comparing the degrees of severity of the epidemic, as it attacked various sections of the community, it was a subject of general observation, that the disease was both more severe and more fatal in private than in hospital practice. The deaths in the higher circles were for a time quite appalling, and scarcely an issue of the local papers appeared without a notice of some death. In the greater number of these cases the head was heavily engaged, and maculæ, delirium, muscular tremors and rapid prostration of the intellect and vital powers, were the usual symptoms. In cases succeeding exhaustion from famine the appearances were very peculiar, the fever assuming a low gastric type, indicated by a dry tongue, shrunk apparently to half its size, and brown in the centre; lips thin and bloodless, coated with sordes; skin discoloured and sodden; general appearance squalid in the extreme, and hunger-stricken. These symptoms, and a loathsome, putrid smell emanating from their persons, as if the decomposition of the vital organs had anticipated death, rendered these unhappy cases too often hopeless. They used to creep about the city while their strength allowed, and then would sink exhausted in some shed or door-way, and often be

found dead. It was hard to ascertain particulars about the course or duration of the disease in this latter class.

"While a general similarity could be traced in the distinguishing features which constituted the epidemic, yet each organ, when particularly engaged, impressed its own peculiar train of symptoms upon the case, so as to cause considerable latitude in its type. Thus, in one form, bronchitis was predominant, and in the aged rapidly assumed an adynamic character: pneumonia and pleuritis were more rare. In another variety gastric disorder existed, and a dry and red tongue, with retching and tenderness of the epigastrium, were the prominent characters, accompanied in severe cases by cæcal gurgling and soreness on pressure, and general tympanitic distention. In others, again, the disease began at once with diarrhœa or dysentery, and many of these cases had been previously suffering from fatigue or contagion. In a fourth variety, the liver seemed the organ principally affected, and jaundice was the chief symptom from the first. This complication occurred more frequently in persons of middle or advanced age than in the young, and did not seem so dangerous as in former epidemics, comatose symptoms rarely taking place. We had then a series of cases showing a resemblance to scarlatina in soreness of the throat and brilliant redness of the tongue, and to acute rheumatism in the pains in the limbs and swellings of the joints, with eruptions on the body of urticaria, or on the legs of erythema nodosum. A curious succession of symptoms was noticed in more than one of the latter class of cases, such as pains in the extremities, sudden transition to the pericardium accompanied by blotches of erythema and soreness of the throat, relief of the heart by treatment, retrocession of the spots, and return of pains to the limbs first attacked, but with abated force. We had finally a large proportion, amounting to one-sixth of the whole, of cases of typhus, all of them attended with severe disorder of the sensorium, with maculæ and early prostration of the vital powers.

"One of the chief peculiarities of the epidemic was the liability to sudden and frequently dangerous changes in the symptoms, especially in aged or weakly persons. You would be suddenly called to a patient pronounced, at your former visit, almost out of danger, and then find the person in a state of total collapse; either the extremities being cold like ice, or covered with a clammy perspiration. Critical changes were noticed in numerous cases. Thus an attempt at a crisis, sometimes successful, would occur in the skin, which, from a dry and burning state, would become suddenly drenched with perspiration. Rapidity of change was also a feature of the tongue, as in a few hours it would vary from a moist secreting state to a dry and brown condition. It could, however, in severe cases, be depended on more surely, as affording signs of an approaching crisis, than any other symptom, being the first to give way. The circulation was very inconstant, being in some persons barely affected all through, and in others running rapidly up, and undergoing an equally rapid depression. Profuse hemorrhage in some instances induced collapse, especially when it occurred from the intestines.

"The ordinary *catamenial discharges* appeared rather to relieve the symptoms, but increased the debility; and in some cases uterine hemorrhage was so profuse that strong styptics had to be employed. In females who were pregnant, abortion commonly took place, and was not attended with as great hazard as in other epidemics, though always a grave complication. In one case, in which the duration of pregnancy was six weeks, abortion occurred on the sixteenth day; in another, advanced to the fifth month, it occurred at an early period of the fever, and in both cases no serious hazard to life occurred; but in the third instance met with, namely, a woman aged 33, who was seven months pregnant, a sudden change for the worse came on the day after the miscarriage, the lochial discharges being arrested, and intense uterine pain, with fluttering pulse, aphonia, and rapid breathing, setting in. By the liberal use of wine,

and of calomel, camphor, and opium, with a few leeches, and the persevering application of the turpentine epithem, she recovered, and was able to leave the hospital a fortnight after.

"*Epistaxis* occurred in my patients about once in twenty cases. It did not come on before the fifth day, and seemed to prefer the uneven days of the fever, viz., the fifth, seventh, ninth, eleventh, and fifteenth. It usually relieved the headach and frontal fulness, and in some cases appeared critical, amendment quickly following. It was noticed in but one fatal case. Its extent was variable, but I did not see any case so profuse as to require interference. The range of age at which it occurred was from 13 to 40, usually about 20, and it seemed more frequent in females. It sometimes co-existed with hemorrhages from other organs, chiefly the intestines, uterus, or lungs.

"*Purpuric spots* were very rare, and usually occurred in children,—one woman, however, aged 50, was thickly covered with deep crimson spots. Traces of the same existed on the palate, and she had profuse evacuations of dark blood from the bowels. She was moribund when admitted.

"*Maculæ* were always indicative of a severe attack, especially when the colour at all declined from the florid state. They were found in cases differing widely in age, from six years up to sixty, and were more frequent in males than in females. The period of their appearance varied from the fifth to the thirteenth day; the eighth was the most frequent. Their period of duration was also inconstant, sometimes lasting but a single day, more usually three or four days, and rarely exceeding six. Occasionally the whole body was coated, but we always found them in greatest abundance on the chest and arms, existing there when absent in other parts. The face was not exempt in the lighter varieties, but showed no trace of the true petechiæ. In the maculated cases, the cerebral and abdominal complications were more frequent than the pulmonary. In favourable cases, the eruption assumed the rose-coloured appearance, fading on pressure, sometimes slightly raised above

the surface, or the rubeoloid character with gastro-enteric rather than catarrhal symptoms. In the cases of typhus gravior there was present either an obscure mottling of the surface or well-marked petechiæ, permanent on pressure, and the rose rash in some instances passed imperceptibly into the latter. In but one case—a boy of 15—was I able to notice *sudamina;* they appeared on the seventeenth day; he had a tympanitic abdomen nearly all through his fever. The symptoms gradually amended after the miliary eruption came out. Herpetic eruptions on the face and lips sometimes occurred, seldom before the fourth day, sometimes on the sixth or seventh. These cases were not always, though mostly, pulmonary. It was regarded by us as a favourable sign, but was in one case followed by aphthous ulceration of the mouth and pharynx, from which the person with difficulty recovered.

"*Delirium* seldom existed to any extent during the first week; it was accompanied by pain of head, vigilia, tremors of the tongue and muscles, &c., and always caused much anxiety, from the disposition in the patient to get out of bed and wander about. In one case the patient got out of a two-story window, and was fortunately caught by the arm while in the act of descent.

"*Relapse* was a peculiar feature of the epidemic, taking place in at least one-sixth of the cases. It occurred mostly in the ages from 15 to 40, being less frequent in children. Old age, however, did not prevent relapse, we had examples of it in women of 60 and 70. The day on which it occurred was variable. It rarely set in before the eleventh or twelfth day from the date of the illness, an interval of a few days usually elapsing between convalescence and relapse. The seventeenth and nineteenth days were not unfrequently marked by secondary fever. The period of the relapse generally lasted from five to eight days, and the symptoms were mostly lighter than in the primary attack; but the reverse was not uncommon. Greater fear of the result was expressed by the patient in the relapse

than in the first illness. It appeared to ensue in some cases from constipation, and in others from errors in diet, the convalescence being marked by a hunger for food, which the patient would satisfy at all hazards. The secondary fever set in with a rigor, followed by vomiting of green matter, tenderness of abdomen, and sometimes syncope, and occurred more frequently after light fever, especially of the gastric type. It sometimes ushered in an attack of diarrhœa, dysentery, or cynanche tonsillaris. Females were more liable to it than males. Some few cases had two and even three relapses, the series becoming lighter, until the disease appeared to wear itself out.

"The *Duration* of the fever was variable. In slight cases, which did not relapse, it terminated in from five to eleven days, while the relapse cases were seldom free of the symptoms before the end of the third, or even the fourth week. The typhoid cases were seldom thoroughly convalescent before the fourth, and sometimes the fifth week. It required a long time for the strength to be regained, pallor of the face and weakness of the lower limbs continuing for many weeks, sometimes for months. No remedy hastened the recovery more than complete change of air.

"The *Mortality* among the 1602 cases admitted into the Infirmary amounted to 156, 93 being males and 63 females; being an average of one death in $10\frac{1}{4}$; of these, however, 36 cases were admitted moribund, dying within forty-eight hours from admission. The mortality among females was less than among males, in the proportion of 1 in $11\frac{1}{3}$ to 1 in $9\frac{2}{3}$. In the spring months the mortality was 1 in 9, while in the summer it was 1 in 12. In the above number are included the cases complicated with dysentery, and the cases of starvation fever, which, when the hospital was first opened, were very numerous. In all instances the heaviest cases of fever were selected from the applicants. From the table of ages, it appears that the results were greatly influenced by age, the ratio being, from 10 to 20,

1 in 24; from 20 to 30, 1 in 12; from birth to 10, 1 in $10\frac{2}{3}$; from 30 to 50, 1 in $6\frac{2}{3}$; and from 50 upwards, 1 in 3. There are no data for the mortality of patients treated in their houses.

"The *Sequelæ* were chiefly erysipelas with typhoid symptoms, coming on generally in the third week of the fever, and lasting from four to eight days, the head and neck being chiefly affected. Blisters to the nape seemed to induce it. Bed-sores were not common. Furunculi followed in several cases, and we had a case of pemphigus gangrenosus attacking the ear in a female. Diphtheritis occurred in some fatal cases towards the close. Dysentery was the most serious result, and, coming on after a severe fever, it proved too often very unmanageable.

"The *post mortem* appearances were noticed, I am sorry to say, in very few cases; but in such as came under my examination a great similarity was observed to the lesions found in the hospitals of Paris; the intestinal glands, especially those of Peyer, being usually either enlarged or ulcerated. The mucous membrane in the vicinity of the glands was in a highly inflamed condition; and in the ileum various degrees of ulceration existed, from slight abrasion to patches of yellow gangrene. Externally the free parts of the small intestines exhibited branching arterial injections, the portions in connexion with the mesentery being less affected. Some parts of the small intestine were much inflated, and others contracted more than natural. In several of the dysenteric cases, the ileum, except in the vicinity of the ileo-cæcal valve, was quite blanched.

"The stomach was frequently found contracted, its mucous membrane corrugated and softened, with congestion; in cases where vomiting was urgent during life, vascularity existed at the œsophageal opening. Both stomach and duodenum were coated with a greenish-yellow tenacious matter.

"The cæcum was nearly always in a high state of inflammation and turgidity, with patches of ulceration of various degrees. The glands in the colon were enlarged, and in dysen-

teric cases the mucous membrane had lost its plaited form, and was completely honey-combed and rugged, the lesion of texture increasing as you approached the rectum: the appendices epiploicæ were changed into a gelatinous appearance. The colon itself was mostly contracted abnormally, and gave to the touch a resisting fleshy feel.

"The liver was found turgid, and loaded with dark blood; the bile in the gall-bladder thin, and of a dirty orange colour; the substance of the liver was easily broken down.

"The spleen was sometimes found hard and hepatized, but more usually enlarged and softened, containing a grumous matter like a mixture of pus and blood. The mesenteric glands were swollen and inflamed, especially in the vicinity of the ulcerated intestines.

"To speak generally of the *Treatment*, there was no disease in which the *nimia diligentia medici* was more hurtful than in this epidemic. The simple cases needed little; emetics of ipecacuanha at the beginning, shaving the head, and keeping it cool, combined with gentle aperient and diaphoretic medicine, conducted the patient safely through. In the gastric complication, sinapisms or blisters to the epigastrium, with effervescing draughts combined with small opiates in mint waters, were generally sufficient in severe cases. Small doses of calomel and opium, hydrocyanic acid, and in some cases brandy, had a good effect. In the typhoid cases stimulants were given early; camphor, ammonia, and ether were useful, but wine was the most to be depended on. We seldom had to exceed three or four glasses in the twenty-four hours. When the sensorium was much affected, blisters were very useful, preceded usually by cautious leeching. The re-application of leeches was preferred to putting on many in the first instance. General bleeding was not employed. In some of these cases, leeches applied to the right hypochondrium had an excellent effect.

"Opium was of the most invaluable benefit; after several nights of restlessness, an opiate produced magical effect. After a trial of more, we seldom exceeded ten minims of Battley, re-

peating it, if necessary, and occasionally adding to it an antimonial. In dysenteric cases, Dover's Powder was our main stay, with opiate enemata. When there was much loss of blood, the acetate of lead and acetated tincture of opium generally checked it, but had less effect on the diarrhœa. Mercury was not given in its specific form, and a difficulty existed of inducing salivation."

APPENDIX TO THE FEVER REPORT ON ULSTER.

From Dr. Seaton Reid, of Belfast, we have received the following Report of the late epidemic, as it appeared in that city:

"It is admitted by all, that when fever hospitals have been in existence for years, and acquired the confidence of the poor, the admissions into them may be taken as an accurate criterion of the amount of contagious disease that prevails in their neighbourhood.

"I find, therefore, on examining the register of the Belfast hospital, that for several months previously to August, 1846, there had been a gradual diminution in the weekly average of admissions, until they had diminished to twelve in that month.

"I think we should date the commencement of the epidemic here from September, 1846, as the weekly average had then risen to 20, and continued steadily to increase in the following ratio:—in October, to 28; in November, to 24; in December, to 31; in January, 1847, to 55; in February, to 50; in March, to 83; in April, to 172; in May, to 350; in June, to 524; in July, to 603; in August it had diminished to 404; in September, to 276; in October, to 199; in November, to 151; in December, to 110; in January, 1848, to 79; in February, to 74; in March, to 59; in April, to 44; in May, to 43; in June, to 34; in July, to 28; in August, to 31; in September, to 23; and in October, to 20, when the average had again reached its minimum number. It is thus evident, that our recent epidemic extended itself precisely over a period of two years, and was one of unprecedented prevalence.

" To meet so rapid and general an increase of disease—our admissions in one week in July being the enormous number of 660—we had three hospitals in operation; and, these being found insufficient to receive all who applied, tents capable of containing 700 convalescents were erected in the neighbourhood of the town. These were found perfectly safe for the reception of this class of patients, provided a little care was exercised in the selection of those that were transferred to them(*a*).

" It could not be said that scarcity of food, or want of employment, was the *originating* cause of our epidemic here, because our working classes were fully employed, and provisions unusually cheap, till the early part of the year 1847. No doubt the reduction in the stamina of our population, by the subsequent deficiency of nourishment, was one of the chief causes of the diffusion of fever, by facilitating the action of contagion, the influence of which in fever will be shown by the accompanying table, No. 3.

" In speaking and writing of the recent visitation of disease it is not unusual to call it an epidemic of fever; this, however, in so far as relates to Belfast, would be very erroneous, inasmuch as the epidemic did not consist in the prevalence of one disease, but was composed of fever, dysentery, and small-pox. Owing to some confusion in the register of one of our hospitals, the relative number of these diseases, with their mortality, cannot with perfect accuracy be now ascertained; but the following is very nearly correct:—Fever, 13,469; died, 1758. Dysentery, 1836; died, 591. Small-pox, 325; died, 103.

" The foregoing were only our admissions into hospital; but I have no doubt that to our fever and dysentery numbers we should add at least one-fourth more, as many were treated at home, owing to the want of room in our hospitals, and the unwillingness of others to enter them.

" The population of the Belfast union, from which the above

(*a*) On the 17th of July, 1847, we had in our tents and hospitals the large number of 2118 cases.

numbers were received, amounted, by the Census of 1841, to 100,992, which in all probability was increased by the poor from different parts of the country to the extent of 110,000, who were attracted to our town by the energetic exertions made by its inhabitants to relieve the sick and destitute.

"I have stated above that the epidemic here was constituted by the prevalence of several diseases, and I now add that in the fever portion of these we had prevailing several separate and distinct species of fever. Twelve years' connexion with fever hospitals has convinced me that a very serious error has been committed by almost all the most recent writers of monographs on fever, in their denying the existence, both in this country and in England, of more than one species of that disease. This error exists in the works of Drs. Southwood Smith, M'Cormac, Ormerod, and others, and will be found pervading almost all the communications in Barker and Cheyne's Report of the Epidemic of 1816–17–18. The consequence of this error has been, that we do not possess that amount of precise information regarding the history, the symptoms, the pathology, and the relative mortality of our several species of fever, that their frequent prevalence in this country would lead one to expect. In this respect the American physicians are in advance of us, for Drs. Bartlett, Jackson, and others, have not only recognised and described the several species of fever peculiar to America, but have also pointed out the difference existing between our maculated typhus, carried into their cities by our emigrants, and their own typhoid fever,—the species of fever so minutely described by Louis in Paris, and which has been so often confounded with our maculated typhus.

"To guard against all misapprehension and controversy, it is necessary to define the meaning attached to the word 'species.'

"Several naturalists, when classifying plants and animals, wished to include under this term all those individuals that bore to each other so close a resemblance that it might be in-

ferred they had sprung from one common stock, but Cuvier and Prichard laid aside mere resemblance as insufficient, and included under it only those that had the power of reproducing an individual having all the essential characters of the parent.

" It is, therefore, in this restricted sense that I would apply the term in fever, and conceive that maculated typhus should be called a species of fever, because infection from it will produce in the infected a fever having the characteristic peculiarities of maculated typhus, and not those of relapsing synocha; and in like manner infection from the species I call relapsing synocha will produce a fever with the characteristic peculiarities of it, and not those of maculated typhus; just as the infection from measles will produce measles, and not scarlatina, and the infection of scarlatina will not produce an attack of measles.

" The question of there being several distinct species of fever in this country is not one of mere scientific interest alone, but deserves our attention for several reasons:—

" First. As each species would appear to have an average mortality peculiar to itself, and as this varies very much in amount in each, it is evident that if a fever with a high mortality prevails in one district of country, and one with a low mortality prevails in another, comparisons favourable or unfavourable may be drawn, which the real facts would by no means warrant.

" Second. An admission of there being several species, each having its own mortality, enables us to account satisfactorily for the very great difference of mortality observed in the hospitals of different localities during the same epidemic, or in the same hospital during different epidemics: for instance, the mortality in the Belfast hospital, among 1258 cases, during the year 1819, was 1 in 32½; while in the year 1838 it was so high as 1 in 7; and in the epidemic of 1845 had again fallen to 1 in 17.

" Third. The question demands our attention, from the direct bearing its solution has upon the lives of our patients; for

if it be ascertained that the risk to life is very much greater from an attack of one species than from an attack of any other, it is evident that we are not justified in treating in the same wards persons suffering from maculated typhus, in which the risk to life is great, with persons who may have attacks of any of the other species in which the mortality is very much lower.

" So convinced was I of the truth of the above as the result of my observation, when one of the attending physicians to the Belfast hospital, then the county fever hospital, that on a transfer of the care of all contagious disease to the poor law guardians, and my appointment as physician to their hospital, I determined to treat maculated typhus in wards separate from other species of fever, and to have separate convalescent wards for each; the result of which has been, that attacks in the hospital of one species of fever after the other are almost unknown.

" But it must not be supposed that the combined prevalence of several species of fever formed a peculiarity of the recent epidemic alone, for, while it is admitted that any one species may prevail at times, to the almost entire exclusion of the others, a reference to the works of Rutty, to the various communications in Barker and Cheyne's Report, to essays in several of the medical journals, and to the printed annual reports of various fever hospitals, will discover such expressions as these:—' typhus fever relapsing again and again;' ' maculæ often present, as also a measles-like eruption; relapses frequent;' ' the fever then changed to a short fever called the five-day fever;' ' the mildest epidemic typhus ever seen; relapses very frequent;'—expressions which clearly indicate that in our former epidemics several species must have been prevalent, but were treated of under the generic term ' fever;' whereas I contend they should have been acknowledged and treated of as separate and distinct species, originating, as I have already remarked, in separate sources of infection.

" I believe that we can in this country have little difficulty in recognising four distinct species of fever, to which I would

propose to attach the following names, the two first being to a certain extent descriptive of the diseases to which they belong: 1, maculated typhus; 2, relapsing synocha; 3, synocha or febricula; 4, synochus, or common continued fever.

" I. *Maculated Typhus.*—I have prefixed the word 'maculated' to typhus, because I believe that there has been no term in medicine which has been used with less precision than the word 'typhus.' So indefinite has been the application of this term in fever, that, perhaps, no two writers for years past would use it as descriptive of the same disease. Whenever, in the course of a febrile affection the tongue became dry, the pulse feeble, and the strength of the patient reduced, with other signs of debility, he was at once pronounced to be suffering from typhus fever. Now nothing can be more certain, as has been well shown by Andral, than that all these symptoms may be the result of some visceral inflammation, and not dependent on contagious fever at all. If, therefore, we wish to use terms which would at once convey to the mind of another an accurate and definite idea of the disease we were describing, we are perfectly successful in this instance, by the combination of these two words, which at once indicate its two leading and characteristic symptoms, namely, the eruption on the skin and the affection of the brain, and describe a species of fever with unmistakeable accuracy and precision.

" It is usually stated that the invasion of this species of fever is slow and insidious, the patient scarcely feeling himself unwell; but I have almost invariably found that he described himself as being on any particular day in a state of the most perfect health, and on the next to have been seized with rigor, headach, pain in the back, and the other usual symptoms of fever. I conceive that the rigor should in this, as in all other species of fever, be taken as the commencement of the disease; and, unless it had occurred during the night, it is almost invariably remarked by the patient. The pulse increases very gradually in frequency, and seldom at any time during its course

exceeds more than 120 or 130 beats in the minute. Even after crisis it requires a few days to subside within the healthy limit, and sometimes descends so low as 35 or 40 beats.

"About the fifth day an eruption resembling measles, which is at first observed on the soft white skin in front of the shoulder-joints, and on the epigastrium, appears on the body; for a few days it is capable of being momentarily removed by pressure, but becomes less so as the disease advances, and produces diminution in the activity of the circulation. Although generally observable till the termination of the disease, it may remain out only thirty-six or forty-eight hours, but is as characteristic of this species of fever as a peculiar eruption is diagnostic of either scarlatina or measles.

"The brain is always more or less severely involved in this fever, especially among the rich and educated classes of society.

"Its duration is generally fourteen days from the rigor, and its mode of crisis either by an increased secretion of urine or gentle perspiration; if the latter be profuse it is in general a fatal symptom.

"The complications occurring during the course of this fever are, in addition to that of the brain, chiefly those inflammatory affections of the bronchial mucous membrane, or of the substance of the lung, which are most frequent among the poor. An abdominal complication is the most rare of any, bearing in this respect a marked contrast to our relapsing synocha or the typhoid fever of Louis.

"There is no pathological appearance peculiar to this disease, whatever changes dissection discovers being only the result of the accidental inflammatory or congestive complications already referred to. In this respect it resembles scarlatina, measles, &c.; and as it appears to be influenced by the six laws described by Dr. Gregory as peculiar to these diseases, I agree with Drs. Stewart, Alison, and others, that it should be placed among the exanthematous fevers.

"A person having had an attack of this disease appears to be protected against a second, as is satisfactorily proved by the well-known immunity of those hospital nurses who have had an attack of it. I am aware persons have been described as having had several attacks of typhus fever, but I have already alluded to the loose way in which the word typhus has been applied, and I believe we have not yet on record an authentic case of a person having a second attack of the maculated typhus I now describe, viz., a fourteen-day fever with the measly eruption and the concomitant affection of the brain. This fever, however, affords no protection against attacks of any of the other species(*a*).

"*Sequelæ.*—This species can scarcely be said to have any sequelæ peculiar to it. If erysipelas prevails in the hospital, a patient having had a favourable crisis may in a few days be seized with rigor and a return of febrile symptoms, which in about three days will be followed by some redness, pain, and swelling about one of his ears, thence extending over the whole scalp, across the face, through the nostrils, into the pharynx, and then into the larynx, producing there one of the most fatal complications that can occur.

"The areolar substance surrounding the parotid gland very often becomes inflamed, passing slowly into suppuration, occasionally producing sphacelation of the coats of the blood-vessels and fatal hemorrhage.

"If dysentery prevails in the hospital, or is epidemic, it may also attack during convalescence, and is in such cases most intractable. The high mortality in some hospitals was caused by this complication; but as at our hospital the dysenteric patients were treated in a separate building, its occurrence with us was comparatively rare.

(*a*) In this opinion we do not agree with Dr. Reid: there are many well-authenticated instances of medical men having had maculated fever twice. (See Drs. Cusack and Stokes' paper on the Mortality of Irish Medical Practitioners.)

"That maculated typhus is a contagious disease appears to me to admit of no doubt; and by a table of the causes of illness assigned by 4601 patients ill with fever, it appears that most were exposed either directly to contagion, or came from establishments in which fever prevailed.

"Of this species of fever we treated during the epidemic 1366 cases, of whom there died 258, which was a mortality of 1 in 5 and a fraction.

"There were admitted, however, during the epidemic, a number of persons in whom there was an eruption on the body, which was either so scanty or indistinct that we could not positively pronounce them to have maculated typhus: many, I am sure, were so, because they came from families in which this species of fever prevailed. I have, however, classified them under the head of *doubtful maculated typhus*, and their number was 179, of whom nine died.

"Finding this communication is extending beyond what I would wish, I shall not enter at all upon the question of treatment, either in relation to this or any of the other species of fever.

"II. *Relapsing Synocha.*—The term synocha has long been used in connexion with fever, and indicated a disease in which the symptoms were in general intense, but of short duration.

"It has also been applied by Dr. Christison to this species of fever, when prevalent in Edinburgh, some years ago; but it has appeared to me desirable to associate with it some word which would form a title as descriptive of it as *maculated* typhus is of another species. I have, therefore, prefixed to it the word *Relapsing*, which describes one of the most marked peculiarities of this fever, and indicates with precision the disease it is applied to. This is evidently the fever which in the reports, &c., already referred to, has been spoken of as 'a short fever,' 'a five-day fever,' 'a typhus fever in which relapses were frequent,' &c. For as it is well ascertained that maculated typhus never relapses in the sense in which this fever does, it is clear that this

is the species of fever to which the above terms must have been applied. The annual reports of our hospital here show that a fever with this peculiarity was not unfrequently observed in former epidemics, and it has now formed a considerable portion of two epidemics since my connexion with the Belfast hospital. It prevailed in Edinburgh in the spring and summer of 1844, and was imported from Scotland in the end of the summer of that year, and formed the great bulk of our epidemic of 1844–45. Doctors Cormack and Wardell have published two able monographs respecting it; and the differences between it and maculated typhus have been very distinctly and ably pointed out by Dr. Henderson, in a paper in the Edinburgh Medical and Surgical Journal for January, 1845.

"I have already remarked that, contrary to the general received opinion, the invasion of maculated typhus is sudden and well-marked; but if possible this disease is still more so. The rigor is most severe and the pulse within twenty-four hours after it becomes as frequent as 120, 130, or 140, and in a day or two after may reach 150 or 160. The headach is in general very severe, but there are not the vertigo and ringing in the ears, with the suffused and injected eye, that are found in maculated typhus. In fact the absence of these symptoms may be taken as a pretty certain indication of the species of fever which we shall have to treat. From the commencement there is very great irritability of the stomach, which rejects everything, and which is accompanied by very great thirst. This gastric complication was infinitely more severe and intractable during the epidemic of 1844, than during our recent one. Most severe pains in all the muscles of the body are very frequently present, rendering the patient almost as helpless in bed as if he suffered from acute rheumatism. Delirium of a very violent and noisy kind often occurs during its course. A very fatal complication in this species was jaundice, which was not unfrequently associated with a true *purpurous* eruption over the body, produced as this species of eruption always is, by a real

ecchymosis or effusion of blood into the areolar tissue, and not, like the eruption of maculated typhus, removeable at any stage by pressure. When these violent symptoms have continued to the fifth day, the patient falls into a most profuse perspiration, often preceded by a rigor, saturating the bed-clothes, and in a few hours the pulse will fall from perhaps 150 or 160 to 70 or 80. No sooner has this crisis taken place than the patient at once feels himself in the most perfect health, and is most clamorous for food and permission to leave his bed. From an examination of a table which I constructed from the hospital register, showing the days, calculating from the period of rigor, upon which a crisis occurred among 385 patients, it appears that on the fifth day after rigor more than double the number occurred than on any other day. Until the extraction of this table from our register I was under the impression that a much larger proportion had their crisis on the fifth day. I may here remark that the pulse and the other symptoms are noted accurately every day in our fever cases, from their admission into the wards till their transfer to the convalescent wards; so that I consider very great dependence may be placed upon the accuracy of this table(*a*).

"The patient having felt himself for some days in the good health above alluded to, has again another rigor, which as the table shows occurs most frequently on the thirteenth, fourteenth, or fifteenth day after his first rigor, or on the eighth, ninth, or tenth day after his first crisis, his pulse becomes again suddenly increased in frequency, with a return also of the symptoms noticed in the first attack, which having continued

(*a*) Dr. Reid has furnished us with a series of most interesting statistical tables, showing "the influence of contagion, &c., in the production of fever, in 4601 patients;" the dates "of rigors and crises in 385 cases of relapsing synocha;" and "the diseases admitted during each month of the year, with their mortality," &c. &c. As the character of our Report does not admit of their publication, we must refer our readers to the last Report of the Belfast Fever Hospital.

for two, three, or four days, a second most profuse perspiration takes place, with an equally rapid subsidence of the pulse; and the patient again feels himself at once restored to health. The table indicates the above days, as those upon which, after the second rigor, the second crisis most frequently occurs.

"I am aware that persons have been described as having a second relapse in this species of fever; but having watched not a few cases for days after the second crisis, without in one instance detecting such an occurrence, I am inclined to believe that in the cases where these three feverish attacks occurred, the patient must have come into the hospital with the synocha or febricula species, to which the relapsing synocha was added. In a few instances the bellows murmur was looked for at the heart, but not detected in any.

"In addition to jaundice, already alluded to, there was another very fatal complication, caused by an entire suppression of the secretion of urine.

"Enlargement of the spleen was occasionally detected in this species of fever, both by the touch and by percussion, but it appeared to have no influence over the result.

"*Sequelæ.*—An attack of this species of fever was occasionally followed by inflammation of the iris, but which appears to have occurred more frequently in Scotland than it did in either of the epidemics here. Amaurosis took place in one or two instances.

"Anasarca of the lower extremities very often followed, and was often accompanied by purpurous spots on the limbs, and effusion into the knee-joints.

"The muscular pains remained for a considerable time after the attack, and pericarditis with effusion was occasionally observed.

"Erysipelas frequently attacked the convalescents in this species also, and dysentery more rarely.

"In no instance have I met with a recurrence of this disease in the same person, although I watched attentively for

it; but, like maculated typhus, an attack of it affords no protection against the infection of any other species.

"During the epidemic, we treated of this species of fever 1014 cases, of whom there died 74, being a mortality of 1 in 13 and a fraction.

"The table which refers to the periods at which the rigors and crises occur during an attack of Relapsing Synocha extends over a period of two years, and gives the dates of occurrence during each month of these years. It is extracted from the reports of 385 patients, and in its compilation all cases were excluded in which dates were uncertain, or in which the youth of the patients prevented dependence from being placed on the history given of their illness. It contains four columns of figures, marked respectively, 1, 2, 3, 4. Column No. 1 refers to the day on which the first crisis occurred after the first rigor, and shows that none occurred earlier than the third, or later than the ninth day; and that in 23 patients it occurred on the third day; in 74 on the fourth day; in 146 on the fifth day; in 74 on the sixth day; in 38 on the seventh day; in 28 on the eighth day; and in 6 on the ninth day.

"Column No. 2 refers to the number of days that elapsed from the date of the first rigor until the recurrence of the second rigor, and shows that none occurred earlier than the tenth, or later than the eighteenth day; and that in 4 patients it occurred on the tenth day; in 19 on the eleventh day; in 33 on the twelfth day; in 77 on the thirteenth day; in 98 on the fourteenth day; in 89 on the fifteenth day; in 35 on the sixteenth day; in 16 on the seventeenth day; and in 11 on the eighteenth day.

"Column No. 3 was compiled in consequence of the possibility of patients not having stated accurately the date of their first rigor; and as the first crisis and second rigor took place almost invariably in the hospital, it was desirable to ascertain if the interval between them was always the same. It shows that in no case did the second rigor occur earlier than the fifth

day after the first crisis, or later than the twelfth day; and that in 9 it occurred on the fifth day; in 19 on the sixth day; in 41 on the seventh day; in 86 on the eighth day; in 99 on the ninth day; in 73 on the tenth day; in 43 on the eleventh day; and in 8 on the twelfth day.

"Column No. 4 relates to the duration of the relapse or second attack, and shows that a crisis occurred in 37 patients on the first day; in 122 on the second day; in 101 on the third day; in 81 on the fourth day; in 31 on the fifth day; in 8 on the sixth day; in 1 on the seventh day; in 1 on the eighth day; in 1 on the ninth day; and none later.

"The deductions from this table are valuable in the management of an hospital during the prevalence of an epidemic of which relapsing synocha may form a part, as they enable us to decide when a patient may with safety be transferred to the convalescent ward.

"For example, if a patient has a feverish attack in which no crisis takes place till after the tenth day from his first rigor, he may immediately on its occurrence be forwarded to the convalescent ward, without being detained in expectation of a relapse, because column No. 1 shows that no crisis occurs in relapsing synocha later than the ninth day.

"The period of safe transfer may also be decided by column No. 2, because this shows that no second rigor occurred later than eighteen days after the first.

"But, as the poor are often inattentive as to dates, the question can be decided with perfect safety to the patient by the results of column No. 3, because the first crisis will in all probability have occurred in the hospital, and should twelve days have elapsed without a relapse the patient may be removed, as this column shows that no relapse occurs later than twelve days after the first crisis.

"III. *Synocha or Febricula.*—This species was the shortest of any of our fevers, and attended with very little risk to life. It commenced, like the others, with a rigor and the usual febrile

symptoms. It had few, if any complications during its course, which generally lasted seven or eight days, nor had it any sequelæ peculiar to it.

"Lest it might be supposed that it was the first paroxysm of the relapsing synocha, I may add that, until the usual period for relapsing had passed by, we invariably took daily reports of the pulse and other symptoms in our cases.

"Of this species we treated during the epidemic 1238 cases, of whom there died 23, which was a mortality of 1 in 53 and a fraction.

"This fever may recur again and again, and protects against no other.

"IV. *Synochus or Continued Fever.*—The initiatory symptoms are the same in this species of fever as in the others. The feverish symptoms are often intense, the brain a good deal involved, and the tongue dry and brown. It is the ordinary continued fever of our country, and its diagnosis depends rather upon the absence of the symptoms characteristic of the other species of fever, than upon the presence of any peculiar to itself. Its duration is uncertain, varying from fifteen to twenty days, and is often complicated with diarrhœa, especially towards its termination.

"It has no sequelæ peculiar to itself.

"Of this species we treated during the epidemic 970 cases, of whom there died 89, being a mortality of 1 in 10 and a fraction.

"I am uncertain whether it may recur in the same person, but it has no power of protecting against attacks of any other species. I very recently dismissed a patient from the hospital who in March, 1847, had an attack of maculated typhus, in July of the same year an attack of relapsing synocha, and in January, 1849, an attack of synochus.

"*Contagion.*—The influence of contagion in producing the fevers of this country is so generally admitted that it might appear almost unnecessary to produce any further proof of its

power; but having recently seen its influence altogether denied by a writer in your Journal, I constructed a table containing the results of my inquiries in 4600 cases, which table also shows its influence during each month of the last two years.

"As the value to be attached to statistical tables depends a good deal upon a knowledge of the method that was pursued in the investigation, I may state that, when writing the first report of each patient's case after his admission, the following questions were generally addressed to him, regarding exposure to contagion.

"'Was fever in your house? Was it among your neighbours? Did you visit them, or did they visit you? Was it in your district or street, and if so, was there any intercourse? Were you in any place where you were likely to be exposed to infection?' And should a negative reply be received to all these questions, the patient was required to account for his illness by being asked, 'Then what brought on your illness?'

"I have known many show great unwillingness to admit that they were suffering from fever, and have therefore not unfrequently been obliged to correct the information received on my first visit, by that obtained some days later, when the presence of fever could not be denied. I have known, for example, a negative reply to be given to all my questions by a person ill with maculated typhus, who had for weeks previously paid frequent visits to the wash-house of a fever hospital, and also to a friend ill with maculated typhus in the hospital.

"When we consider how very possible it was for persons during an epidemic prevalence of fever to be exposed, without their knowledge, to contagion, by meeting persons from houses in which fever existed, in shops, or other places; the number unable to assign exposure to contagion as the cause cannot be looked upon as large, and the influence of contagion as the chief cause in the propagation of our several fevers, must, I think, be at once admitted.

"It would be a dangerous statement to deny the possibility

of the origin of fever *de novo*, because the same Power that created the combination of circumstances under which it first appeared among men, must of course be equal to their renewal; but when a case of fever has occurred, its capability of being communicated by the sick to the healthy appears to me to admit of no doubt, and such an occurrence can be prevented only by the energetic adoption of measures to insure extreme cleanliness, free ventilation, and frequent changes of bed clothing, the great influence of which in limiting the disease to one member of a family is well exemplified among the richer classes of society, where such measures can be efficiently carried out.

"Of 4600 patients treated for fever in our hospital during the late epidemic, I find that in 1496 suffering from *Maculated Typhus*, 940 were exposed directly to contagion; 131 came from the workhouse, where fever prevailed; 40 from the night and day asylums (institutions established for the relief of the strolling poor), and where fever also prevailed; 17 came from streets or districts where fever prevailed, but said there was no intercourse; 79 assigned cold as the cause; 170 could assign no cause; and in 119 the cause was not noted in our reports.

"In 965 patients suffering from *Relapsing Synocha*, 694 were exposed to contagion; 92 came from the workhouse; 38 from the night and day asylums; 2 had fever in their neighbourhood, but without any intercourse; 28 assigned cold; 67 no cause; and in 44 reports the cause was not noted.

"In 962 afflicted with the *Synochus* or common continued fever, 530 were exposed to contagion; 183 came from the work-house; 36 from the night and day asylums; 10 had fever in their neighbourhood; 27 assigned cold; 78 no cause; and in 98 the cause was not noted.

"In 1177 patients suffering from *Synocha or Febricula*, 567 were exposed to contagion; 231 came from the workhouse; 75 from the night and day asylums; 9 had fever in their neighbourhood; 64 assigned cold as the cause; 162 could assign no cause; and in 69 the cause was not noted in our reports."

From Strangford, county of Down, Dr. Alexander Knox writes:—

"Fever was epidemic, in the spring of the year 1847, in the Strangford dispensary district, containing a population of about 5000 souls. Dysentery had also been epidemic from the preceding year, and continued to prevail simultaneously with the fever; but the persons attacked by the latter disease were, for the most part, apparently in their ordinary state of health. Neither purpura nor scurvy was observed by me in any case; nor, except in one instance, was the fever complicated by the supervention of dysentery. As far as I could ascertain, there was no concurrent epizootic disease. From the number of children in most houses, they appeared to suffer in a greater proportion than the adult population; but no peculiar exemption was enjoyed by either sex, or by any age. As the lower classes of the people in this district were much better circumstanced than in most others, with reference to the provision of employment and food for the people, it was my good fortune not to witness a single case of the epidemic which could be attributed to starvation. I have not the slightest doubt of the disease being contagious; and I have long since so completely satisfied myself on this point, from repeated observations in different epidemics of fever in this country, that I look on it as perfectly ascertained, and take my measures for prevention accordingly. At the same time it is right to state that I do not consider contagion to be the usual, nor even the common mode of the propagation of fever, but I believe that it depends on malific influences, of whatever kind, infinitely more general and speedy in their operation.

"On comparing the fever of 1847 with former epidemics, the points of difference that struck me, as far as the disease fell under my own immediate observation, were the shortness of its course in a few, and its comparative mildness in the generality of cases. In illustration of this fact I may state that of fifty dispensary patients affected with fever not one died, although some of them

had the disease in a very grave form. One patient, with a pulse that I calculated to be about 200, for I could not count it, recovered completely. This man, a schoolmaster, has had fever three times; and his case, and many others in my practice, appear to be a sufficient answer to the notion that typhus attacks the same individual only once in his life. Purpuric spots occurred in about one-third of the cases. The brain was the viscus most frequently affected, deep somnolence being a very common symptom. Disease of the lungs was the complication next in frequency. Epistaxis was rarely if at all observed; and the same may be said of bed-sores and enlargement of the spleen. Relapse, *contrary to what so generally took place in many other districts, did not occur in a single instance.* A difference in the intensity of the disease was very observable, even in members of the same family, in some the symptoms being very slight, in others extremely grave. I have not witnesssed any sequelæ, the recovery being complete. As regards treatment, I only found it necessary to use leeches in two or three cases, and venesection once, with very obvious benefit. Beyond regulation of the diet and the bowels, enforcing cleanliness and ventilation, and the use of diluent and diaphoretic drinks, little treatment was necessary in many cases. Blisters were applied to the nape of the neck, and the scalp was ordered to be shaved, where there was profound cerebral affection. Mercury, except as an aperient, was only used in one case, where inflammation of the brain was present. Opium was unhesitatingly employed after depletion, and found very beneficial where much nervous irritability and sleeplessness were prominent symptoms. Stimulants were rarely resorted to, and the results showed that they were not necessary; in two or three cases only did they seem called for, and then wine and water was administered at regular intervals, in moderate quantities. As far as my observation extended, large doses of wine or brandy were neither necessary nor should I have considered them safe. The lower ranks, from their ordinary regimen, seem to me to be very susceptible of slight stimuli in fever, and could not

well bear, I apprehend, the same line of treatment that might be necessary for the habitual wine-bibber. I have now had pretty extensive experience in five or six epidemics of fever in this and the adjoining county of Antrim, and in none of them have I had occasion to resort to the large quantities of stimulant drinks described by some writers on fever as indispensable. I believe the fever of 1847 was more fatal amongst the better classes, in proportion to the numbers affected; but I have no sufficient record of cases to deduce from them any statistical inferences of value."

APPENDIX TO THE FEVER REPORT ON LEINSTER.

We shall conclude this Report with the following observations on the epidemic fever of 1847–8, by Dr. Robert Law, Queen's Professor of the Institutes of Medicine and of Clinical Medicine in the School of Physic in Ireland, &c.:—

"I regret that I have been hitherto prevented, by circumstances over which I had no control, from answering the queries which you addressed to me in a letter dated August 28, 1848, respecting the epidemic fever that prevailed in this country in 1847 and 1848. I especially regret it lest I should be thought from my silence to undervalue the information to be obtained from such inquiries. I believe that the imperfection of medicine is more attributable to the want of faithful records than to any other circumstance, and that nothing would tend more to its advancement, both as a science and as a practical art, than the recorded observation and experience of disease and its treatment. The medical profession might take a useful hint from the profession of the law in this respect. The numerous volumes of reports indicate the degree in which the legal profession values such information. When a new case presents itself to the lawyer, he has his 'Reports' to apply to, and then can compare the features of his case with others, see how far they agree or differ, and thus ascertain, in legal parlance, 'the law of the case.' Similar cases are similarly adjudged. If medical reports were as full and complete, the phy-

sician could know where to apply when any new outbreak of a disease presented itself. But unhappily such is not the case. And in nothing is this deficiency more apparent than in the scanty records that we have of the epidemic diseases which have at various times visited this country, and of which so little notice has been taken that on the appearance of a new epidemic the physician has but little aid from the experience and observations of others exercised on former occasions, but is thrown on his own resources, which to any individual, no matter how favourably circumstanced for such a task, must be comparatively limited, and can only embrace what has occurred in his own immediate locality, and, therefore, want that variety and diversity derived from variety and diversity of locality, &c. It is the contribution of individual experience from different localities, exhibiting the special circumstances and modifications of disease in each locality, that medicine requires as the material to raise it, as a noble science and a useful art, to that elevation to which its high objects entitle it.

"The mode that you have suggested for obtaining this object, by submitting to the members of the profession, differently circumstanced and located through the country, a number of judicious and well-chosen questions, is admirably calculated to produce the effect; and I trust that it will issue in leaving on record much valuable information, which may afterwards be referred to with great advantage, both in regard to the nature and character of the epidemic as well as to its treatment.

"It is incumbent on those who are jealous for the credit of medicine, and who would vindicate it from the imputation with which ignorance so often assails it, of uncertainty in its practical application, to have the published records of their observation and experience to refer to in proof that disease so changes in its nature and character as to require corresponding change of treatment; and that, if medicine is to be, as it ought to be, and as it is competent to be, a real blessing to mankind, it ought to know how to free itself from the fetters that system

would impose upon it, and with versatile aptitude accommodate itself to disease, which is and must be as various in its nature as the elements out of which it arises, and, therefore, require as various treatment. Of the fact of a revolution in the character and nature of disease every one who has had opportunity of observing it for the last twenty years must be aware; it may be said to have undergone within that period a complete change. We confine our observations to our own experience; and having been constantly in the same locality, and being during all that time connected with a large hospital, we have been favourably circumstanced for observing the various changes disease has undergone. The first epidemic fever of which we had personal experience was in 1826–7. Its type was essentially sthenic; it generally ran its course in seven days, and usually terminated by a distinct crisis, preceded by the characteristic *perturbatio critica*. The most common critical discharge was perspiration, the next most common was epistaxis, the rarest was intestinal hemorrhage. At this time petechiæ were very rare, and, from having been hitherto only observed in cases associated with dangerous symptoms, created considerable alarm; so that to pronounce a person in 'spotted fever' was regarded as an announcement of almost certain death. The subsequent frequency of this phenomenon, and in company with comparatively mild symptoms, deprived it of the terror that had till now attached to it. The treatment applied to this fever was more or less decidedly antiphlogistic.

"Perhaps there was no physician who had a higher reputation for his judicious and successful treatment of fever than the late Dr. John Crampton; and his almost constant practice in this fever was to bleed from the arm, even when there was no complication of local inflammation. Among those who were opposed to bleeding some carried on a daily system of purgation, while others directed their remedies more particularly to the skin. This type of fever prevailed until the cholera visited these countries in 1832.

"Before this period the influenza was little more than known by name. Its desperately overpowering influence had not hitherto been felt. It now spread in a way in which it had never been known to do before, and assumed a variety of shape and form quite new to our experience. Its victims were calculated as more numerous than even those of the deadly cholera.

"Hitherto scarlatina had been a comparatively mild disease. Prudence, more than the urgency of its symptoms, often made us impose on our little patients a restraint of which the little inconvenience that they felt from their slight indisposition made them impatient; nor could they understand why they should be confined to bed. But now it assumed a character of violence to which we had hitherto been utter strangers, sweeping away entire families of children, and not sparing adults. Nor was this malignity measured either by the sore throat or the extent of the eruption; it was the fever that accompanied it that stamped it with its dangerous character. In many of the worst and most rapidly fatal cases there was scarcely any sore throat or eruption. In some cases there was no sore throat, while there was a cutaneous efflorescence; in other cases again there was sore throat and no eruption. We met with one remarkable case where there was neither eruption nor sore throat, but in which the fever identified the disease with the fever of the scarlatina then prevailing. We therefore inferred that, as with measles, the eruption is not essential to the disease, so in scarlatina the disease might be present without either sore throat or eruption.

"Up to this time small-pox in persons vaccinated was so rare that it was thought that the disease, when it did occur, must be due to the operation of vaccination not having been perfectly and carefully performed. The fallacy of this view might have suggested itself, even when the cases were rare, from the difference between the disease in the vaccinated individual and the natural small-pox, a difference which would

not have existed had the operation been imperfectly performed. But when, as afterwards occurred, small-pox spread as an epidemic, then the most sanguine advocates of the protective power of vaccination were constrained to admit that they could no longer charge the disease on want of due care in the operation of vaccination. My own conviction is that the protective energy of vaccination was weakened by the mysterious change that occurred in all diseases at the time.

"From this period also a remarkable change took place in what we call idiopathic fever; and, although it presented itself under a great variety of aspects and phases at different times, it never assumed the character under which it appeared previously to the appearance of cholera. Since then it has been invariably of a low, typhoid, adynamic type. It has always run a more protracted course than it did before, nor has it ever exhibited what could be termed a decided crisis, except in the last epidemic, where the appearance of crisis in a few instances made us imagine that the cycle of disease marked by a low typhoid or adynamic character having completed its course, we were returning to what we had before been accustomed to. This view, as we shall afterwards observe, was but very partially true. The usual progress of fever, since the period to which we have alluded, has been, for the symptoms daily to increase in intensity, and daily to assume a more alarming aspect, till they had attained their *acmé*, and then either terminate fatally, or as gradually abate somewhat of their severity, and the patient thus creep through a tardy convalescence. While the fever has ever been true to its adynamic or typhoid type, it has exhibited much variety in the secondary lesions that it has presented in various epidemics. The most remarkable among these secondary lesions has been the intestinal affection, whose frequency and intensity have induced many pathologists, especially on the Continent, to believe that what had hitherto been considered idiopathic fever is a mere figment; that all fevers are symptomatic of some local lesion; that the designa-

tion "typhus fever" ought to be expunged from nosology, and its place be occupied by enteritis, mesentero-enteritis, follicular enteritis, dothinenteritis, &c. The Irish pathologist, who, perhaps, from the circumstances and impoverished condition of that class amongst which fever is seldom absent, is placed on a vantage-ground for learning by continued experience how far any local lesion is so constant as to be entitled to be regarded as the essential element of fever, still maintains the distinction between idiopathic and symptomatic fever, regarding the local lesions as merely accidental complications, and often not present. Thus has he, if not often, at least sometimes, found that in an epidemic fever, whose prominent character was the intestinal lesion, if death occurred within a few days after the seizure, which it occasionally did, in such cases he in vain looked for evidence of disease in the intestine; it was not to be found. He did not even find that peculiar projection of the mucous membrane corresponding to the groups of enlarged subjacent follicles, which constitutes the early stage of this complication, and which, while it exhibits itself in this form in the portion of the intestine nearest to the stomach, acquires a more decided inflammatory character as we pursue the tract of the intestine till we come near the cæcum, where we generally find extensive ulceration. In fact death came on too quickly to allow the intestinal lesion to develope itself. They, then, who maintain the dependence of the fever on this lesion, must, if they are honest, allow that in some cases it is not present, and that it therefore wants the essential character of the *causa proxima quæ præsens morbum facit, sublata tollit*, or else they must abide by the alternative that the effect precedes the cause, and in some cases the cause has not appeared at all(*a*). That this intestinal lesion

(*a*) Rokitansky, while he maintains the dependence of typhus on the intestinal lesion, confesses that sometimes there is no such lesion to be found, but relieves himself of the embarrassment that such an apparent inconsistency would create with an ordinary person, by the gratuitous assertion that there may be an arrest of development of the local affection, or that it may run its entire course in the blood. *Credat Judæus!*

is very frequently present in fever we readily admit, but we only allow to it the place and importance that we give to other lesions that we also found to complicate the original fever, that of being a mere accident, although strikingly modifying the original disease.

"Among these secondary affections or accidental complications, the most conspicuous was the intestinal, either alone or alternating with cerebral affection. This complication was often found to be extremely embarrassing, and required to be treated with much care, for the relief of one set of symptoms aggravated the others, so that the sudden check to diarrhœa seemed to concentrate all the energy of the disease on the brain. At other times the head alone was affected, and perhaps it was now we were first taught that stimulants, especially wine, so far from being contra-indicated in fever with cerebral excitement, even in its earliest stage, were actually required. We had already learned that the delirium which comes on in the advanced stage of fever, when debilitating causes have had time to operate, requires to be very differently dealt with, from the same symptom appearing at an early stage of the disease, and then we account for it by charging it upon what we term determination of blood to the head. But to meet this symptom with stimulants, when it appeared at nearly the commencement of fever, was a new point of practice.

"Affections of the respiratory organs, either as bronchitis or pneumonia, now also frequently presented themselves as a complication. The pneumonia complication, at least under a particular form of it, was a very grave one, and not unfrequently ended fatally. We allude to cases where the lungs became almost all at once solidified, not as in common pneumonia, where it, as it were, dwells upon the first, or hyperæmic stage, and then gradually runs into the second stage, or stage of hepatization. But here it appeared as if all at once the disease hurried into what is called the third stage. A remarkable feature of this pneumonia was that it affected more frequently the upper part of the lungs.

"The first case that occurred to us exhibiting the dull sound on percussion under the clavicle, and the characteristic muco-crepitant râle, made us suspect that fever had attacked an individual already labouring under phthisis pulmonalis. And it so happened that the individual was not in a condition to give us much information on his previous health. However, although his fever was an extremely severe one, yet he recovered; and as the febrile symptoms disappeared, and his condition improved, the lung affection also shared in the amendment, and ultimately left no trace of disease behind. This was conclusive proof that there was no phthisis here. As this local lesion was one indicating the most asthenic type of disease, it is not to be wondered that we had opportunity of ascertaining by *post mortem* examination what its real nature was, and found it to be what Laennec designates, but, I conceive, erroneously, the third stage of pneumonia. My objection to Laennec's regarding this peculiar pathological condition of the lung as the third stage of pneumonia is, that it conveys the impression that the disease has regularly passed through the first and second into another stage, and that this is its normal course. This is what we do not believe. As little do we consider it the natural tendency of adhesive inflammation in the external areolar membrane, to run into what we call diffuse inflammation, as we consider it the natural tendency of hepatization of the lungs to run into this suppurative pneumonia. We, in fact, regard this pneumonia as essentially diffuse inflammation of the lungs, agreeing with diffuse inflammation elsewhere in its anatomical characters, these consisting in purulent infiltration, not, as asserted by Laennec, into the pulmonary structure, but into lymph deposited in this structure. This species of pneumonia also occurs in a diseased condition of the system, analogous to that in which diffuse inflammation in other parts is met with.

"Bronchitis, under various forms, constituted a very frequent complication of this fever. In some cases it presented as capillary bronchitis, and exhibited itself at a very early

period of the disease. We have known instances where the similarity of its signs to those of pneumonia, as well as the congested face, has led to the mistake that it was pneumonia, and, therefore, ought to be treated by depletion, by bleeding, a practice which was attended with the most disastrous results. If measles happened to prevail at the time that fever also was rife, while the fever of the exanthema assumed the type of the prevailing epidemic, it did not exhibit the peculiar local complications of the fever at the time, but presented its own bronchitis, under circumstances of peculiar aggravation. Often was the fever prolonged by the bronchitis, which in many cases also continued long after every febrile symptom had subsided, and, terminating in protracted bronchorrhœa, frequently made us fear that it would issue in phthisis, an apprehension that was extremely rarely realized. The inhalation of turpentine vapour almost invariably succeeded in checking the excessive bronchial secretion.

"While the diseases that come more immediately within the observation of the physician were seen to have undergone this remarkable change of type, the same change of character was noticed amongst surgical diseases. This particularly struck the late Mr. Colles, than whom no one observed disease with more accuracy and sagacity. He remarked to the author of these observations how rarely he met with disease that required or admitted of the use of the lancet; to use his own expression, he missed the "honest inflammation of former times." This change exhibited itself in the low typhoid erysipelas that appeared epidemically from time to time. Now also diffuse inflammation, under its various shapes, was more common, as also phlebitis, purulent arthritis, and all the category of diseases which are, *par excellence*, designated diseases of constitutional irritation. About this period we met with a remarkable case of purulent pericarditis, with the external and constitutional symptoms of diffuse inflammation. There were the pustule and swellings, with the blush of inflammation, about the joints.

The case seemed to us to establish a relation between purulent arthritis and rheumatism; it suggested to us that they were the same disease modified by the fever, which was typhoid in the former.

"The routine of disease was now interrupted by the appearance of a new malady in this country and Scotland, and partially in England; we allude to purpura or scurvy. This disease was evidently connected with the change that ensued on the failure of the potato. It in all respects resembled the disease met with among sailors, and in most cases yielded to exactly the same treatment.

"There was a peculiarity that marked some cases of fever that occurred at this time, which consisted in a group of symptoms pointing to an affection of the brain and spinal marrow, thence designated cerebro-spinal arachnitis. There were few medical men connected with hospitals who had not occasion to observe one or more of such cases. These cases were very novel, and at first embarrassed the physician, presenting themselves under an aspect little calculated to throw light upon their exact nature. My friend Dr. Darby first noticed the disease in this country; the cases that he first saw resembled cases of cholera. Of four cases that occurred to me, three laboured under constant vomiting, and exhibited some features of cholera.

"I now come down to the fever which forms the subject of your inquiries, and shall direct my attention to and furnish answers to those questions that marked that fever. I never saw one exactly similar. As I observed already, the appearance of a crisis by perspiration led me to believe that we were returning to the fever with which I had been familiar in our early practice, but of which I had completely lost sight. However, the cases of crisis were extremely few, and most frequently the crisis was not complete, and seemed only to lead to a mitigation of the symptoms; and it was not till after several abortive efforts that anything like a complete crisis took place.

"The prominent symptom of the disease was an overwhelming feeling of prostration of strength and depression of spirits. In this respect the disease resembled aggravated influenza. The fever was essentially of an asthenic or adynamic type. I never saw any fever in which the advocates of the theory of the idiopathic nature of this disease had more countenance for their view, in contrast with those who maintain its constant dependence on some local lesion. These latter would have been puzzled to find out an organ, or set of organs, upon whose disease to hang the fever. It seemed as if the morbific influence or agent did not, as in former epidemics, concentrate itself upon any particular organ or apparatus, but assailed all, spreading itself over all, and exerting over all a debilitating effect. This may account for the small mortality, as the *onus* being shared by many made it more tolerable than if a single organ had to bear all the burden of the disease. In the midst of this small mortality we met with cases which ran their fatal course in a shorter time than we had ever before seen.

"In one case, from its commencement to its fatal termination, the disease only occupied four days. The case to which we allude was a man aged forty years, of a full habit, who exhibited a stupified aspect, like one in a state of intoxication; his eyes were deeply suffused. The surface of the body was of a greenish yellow hue, the colour of tallow. The pulse was scarcely to be felt at the wrist, and the stethoscope indicated an extremely feeble action of the heart. No stimulants, either applied externally or given internally, appeared to produce the slightest result or response. The effect in this instance resembled that of a strongly concentrated poison, acting with overwhelming power on all the energies of the system, and deadening them to every stimulus. Here, if the morbific impression was made on all the system alike, all was equally overpowered by it.

"Our observation, that on no one system more than another did the morbific influence act, requires some qualification. The most common and the loudest complaint of our patients was an

aggravation of what the subject of fever generally describes as pains in the bones, but which really means pains in the course of the spinal nerves. In former fevers this complaint was seldom more than that of a contused or bruised feel, or of such a sense of discomfort or fidgetty restlessness as the French so significantly express by the term *malaise.* Here, however, the individual in many cases seemed to suffer as intensely as in the severest case of acute rheumatism. These pains were more or less general in different patients. In some they affected the back of the head and neck; in some they only ran down the legs; while in others they spread themselves through the whole body, and, embracing the sides, imparted the sensation of painful constriction. The nape of the neck and across the loins were the points of the spine to which the patients most frequently referred their pain. When it occupied the former position, which it did in many cases, we regarded it with some apprehension, from its being the starting point of that formidable disease, cerebro-spinal meningitis, into which we are sure it would have passed had it not been arrested by treatment. The means we employed to relieve these pains, and which were attended with constant success (indeed we would say that there was not a single case in which they did not fulfil their purpose), was to apply leeches in such numbers to the spine as the extent of the pains required. If the pains extended to all the body, and to both the upper and lower extremities, the leeches were directed to be applied all along the spine. If the pains were confined to the upper or to the lower extremities, then the leeches were only applied to the upper or lower portion of the spine. In some cases, in order to prove the effect of the leeches, we applied fewer than the extent of the pains required, and found that there was a relief to the pains in the parts supplied by nerves from the portion of the spine to which the leeches were applied, while in the other parts they remained in *statu quo*, and waited for the application of the leeches to their portion of the spine for their removal.

"It had long been our practice in former fevers to apply leeches to the nape of the neck in cases of occipital headach, and to the loins when patients complained of pain in this situation, which they so often do. We had often, in our clinical instruction, pointed our pupils' attention to the localities best situated for the application of leeches to relieve the different kinds of headach of which patients complain in fever, a point which we felt had not been sufficiently attended to by practitioners. Thus we taught them to apply them to the nose when the patient complained of supra-orbital headach, which is almost the constant complaint in influenza; to the temples in case of throbbing of the temples; behind the ears when the patient complained of a sense of weight and fulness of the head generally; to the nape of the neck when the pain was referred to the back of the head; and, again, to the loins when the pain was complained of here. In this last situation we do not believe we ever saw leeches applied to relieve the pain so often complained of in fever. Nor could we understand why, for certainly it is often severe enough to render its removal a great relief to the patient. We suppose the reason why the headach commands attention, while the lumbar pain is overlooked and left to itself, is the reasonable apprehension lest the brain should suffer, and therefore art must interfere and throw its shield round this important organ, while there is no such apprehension for the spinal marrow. If this be the reason of the difference of treatment of the two complaints, we have not acted upon it, ever considering the relief of the lumbar pain, if at all severe, motive enough to justify the application of a few leeches, which could do no harm. But we did learn from a case which recently occurred to us, that if headach required the interference of art to prevent the brain receiving damage, the pain of the back also required looking after, lest the organs more or less intimately related to it should suffer. The case to which we allude was that of a young man, John M'Cushion, who, when admitted into hospital with slight febrile symp-

toms, complained chiefly of pain across his loins, for which we directed leeches to be applied, which completely removed it. He appeared to be getting quite well, and was placed on diet, when he again complained of pain in his back, but so slight that we did not think it necessary to repeat the application of leeches. The skin presented a slightly jaundiced hue. The next day we found him deeply jaundiced; pulse 180 in the minute. The nurse told us he had been raving all night. He was now in a heavy, lethargic state, approaching to coma, and did within a few hours pass into profound coma, which speedily terminated in death.

"Examination of the body exhibited the liver and spleen much enlarged in size and congested, and so softened in structure, that they seemed as if they had been soaked in blood. Their condition was such as seemed to warrant us in regarding the case as apoplexy of these two organs, a term perhaps more strictly applicable to their condition than the corresponding state of the lungs to which it is now so generally applied. The suddenness and violence of the seizure certainly identified it more with cerebral apoplexy, and therefore gave it more title to the designation of apoplexy than belongs to the pulmonary lesion, which seldom shows itself with such suddenness or violence.

"Although this was the only extreme case of this kind that occurred to us, we did meet with so many cases of dull pain in the left hypochondrium with dulness on percussion, and in which the stethoscope proved the integrity of the lung, that we had no doubt of the frequency of congestion of the spleen. Nor was this the only feature of resemblance that we thought we discovered between this fever and ague. We do not mean that it had anything of an intermittent character, but in its affecting the ganglionic and spinal rather than the cerebral portion of the nervous system. This view led us to adopt a modified treatment, which seemed to succeed in the cases in which we

employed it. This modification consisted in the combination of James's Powder and quinine.

" As we never before witnessed anything to equal the severity of the pains complained of in this epidemic, so we never witnessed anything in medicine more signal or striking than the relief afforded by leeches applied to the spine in such cases of what we would designate pseudo-rheumatism. An instance of this was exhibited in the case of Anne M'Guinness, aged 25. She, with her sister Mary M'Guinness, aged 20, was admitted into hospital. The younger had had a very severe fever in Cork-street Hospital, three months before, and had suffered much from a bed-sore occupying an unusual position, namely, the left side of the body, below and external to the left mamma(*a*). Her fever on this occasion was of a much lower type than that of her sister's, which was of the mildest kind, requiring only some diaphoretic medicine. In the course of the night of her admission, however, she was seized with most violent pain along the vertebræ and the loins, and extending down the legs. The next morning we found her in great agony from these pains. We directed leeches to be applied to the spine. They afforded her almost immediate relief. She recovered rapidly, and left the hospital sooner than she ought. She soon returned to us in fever, and suffering as severely from the same kind of pains as before, but situated higher up, extending along the side of the neck and the arms. The recollection of the relief she had formerly obtained from the leeches made her most urgent that

(*a*) " It is an interesting question connected with the statistics of fever, how far one seizure renders the system more or less susceptible of a second. If it could be established that one attack did secure the system against a second, the fact would favour the view of the pathologists who are disposed to regard typhus fever in the light of one of the exanthemata. Although we are not amongst those who hold this view of the nature of fever, we do believe that one attack of it does, to a certain extent, fortify against a second. How few medical men have not had their seasoning?"

they should be applied again, which was speedily done, and with the same relief as before. This was equally remarkable in the case of a young man, named John Chittim, aged eighteen years. He had gone out of hospital after having had a protracted fever, but not marked by any peculiarity except the characteristic prostration of the epidemic. After being some time out he was re-admitted with pains more extensive and more acute than we had ever witnessed in the severest attack of rheumatic fever. The head alone was not affected, but both the upper and lower extremities and sides of the trunk were. The pains in this last situation he compared to the most distressing sense of painful constriction. In this case I directed the leeches to be applied to the spine in two batches, in order to see the effect, and found it just as we expected, the leeched parts relieved, and the unleeched parts as painful as before. We found tincture of iodine applied to the spine, and along the course of the pains, nearly as efficacious in relieving the pains as the leeches, but not to the full extent. We were quite sure of the one, as we did not experience a single case of their failing to yield the most decided relief. The low asthenic type of the fever might have led us to suppose that the application of a large number of leeches would have produced great weakness and debility, but this did not prove to be the case. And this strangely contrasted with the great prostration that ensued from our taking so small a quantity as six ounces of blood from the arm of a man in the fever. The circumstances under which the blood was drawn were as follow:—MM. Rodier and Gueneau de Mussy, who came to this country commissioned by the French government to investigate the prevailing fever, accompanied me in my visits at Sir Patrick Dun's almost daily. M. Rodier, to whom medical science is so much indebted for the information he has contributed on the changes the blood experiences in disease, was especially anxious that I should bleed a patient to furnish him with the means of prosecuting his favourite inquiry. It was a long time before I met with a

patient in whom I could feel myself justified in yielding to his request.

"At length there was a strong, healthy man, in whom the disease seemed to exhibit somewhat less of its low depressing type, from whom I thought I might, without running any risk, allow a small quantity of blood to be taken. Accordingly, I directed six ounces to be drawn from the arm. This was done at two o'clock in the afternoon, and at nine o'clock in the evening, the resident apothecary, when making his ordinary visit, found him so low and exhausted that he was obliged to give him brandy to prevent him sinking from exhaustion. When I saw him the next morning I found him in a state of extreme prostration; his pulse very weak and compressible. He exhibited the pallid, sallow, subicteric complexion, characteristic of hemorrhage. We were obliged to continue to give him brandy and wine, and at length, after a tedious convalescence, he quite recovered. I felt I had done enough (*sat superque satis*) for science. It was the last venesection I allowed to be performed on any of my fever patients. The experiment strikingly illustrated how different are the results of local and constitutional bleeding. For although it might be urged that depletion by leeches might have been followed by the same degree of exhaustion in this case as that consequent upon the venesection, or there might, perhaps, have been some peculiarity or idiosyncracy in the individual, we can only assert that the appearance of the individual was such as to make us believe that no injury would be likely to result from taking from him a small quantity of blood, much less that he should be almost completely prostrated by no greater loss than a young child would, under common circumstances, bear with impunity. And to suppose that an equal hemorrhage from leeches might have produced the same degree of exhaustion would have been contrary to the analogy of the numerous other cases in which we had occasion to employ them without their leaving any sense of weakness behind. We cannot help

remarking upon the change of complexion that ensued upon the very moderate venesection, proving it to be something more than a mere transient nervous feeling; that it was a real change produced in the constitution of the blood, in which the physiological relation between the hæmato-globulin, or red particles, and serum, was altered at the expense of the former; that this fluid had undergone the ordinary change that it does undergo in extensive hemorrhage, chlorosis, malignant disease, &c. The effects of this experiment proved, in addition to every thing else connected with the disease, its extremely asthenic type.

"Almost the only remedies we thought of employing were stimulants in some form or other. The most common stimulants were either brandy or wine, with our ordinary cardiac mixture, composed of camphor mixture, carbonate of ammonia, and Hoffman's anodyne. We generally ordered diaphoretics, viz., James's Powder, water of the acetate of ammonia, &c. In this respect this epidemic differed from all preceding ones, that there was scarcely a case in which we could not freely give either brandy or wine. Every practitioner of any experience in fever knows in how many instances he feels wine or brandy to be almost the *unicum remedium* for his patient, yet the remedy cannot be borne, from the amount of nervous disturbance that it produces, and which renders its continuance quite inadmissible. But rarely did such a case present itself to us in this epidemic, and in consequence its treatment was extremely simple. We never met with an epidemic more calculated to sustain the pretensions of the Brunonian theory of fever. Some few of the cases were more like what Dr. Armstrong described as congestive fever than any we had hitherto met with. This resemblance exhibited itself in congestion of the surface, in a kind of cyanosed appearance of the body, not like either the effects of petechiæ or maculæ, but what we observe as the effects of intense cold While this was the state of the surface, the respiration was laboured and oppressed, and the stethoscope indicated, by the

presence of crepitus, the existence of pneumonia, or perhaps more properly congestion. Did the presence of pneumonia or congestion cause us to modify our treatment, or to withhold, or even to lessen the amount of the stimulant? By no means. We gave the stimulant at least as freely as if there was no pneumonia, and witnessed as the result both the disappearance of the lividity and congestion of the skin, and a most decided relief to the oppressed breathing, and removal of the crepitus. A striking case of this kind occurred in a youth, John Hackett, aged 18. There was universal blueness and congestion of all the body, and of both the upper and lower extremities. There was, at the same time, dyspnœa, and the stethoscope indicated crepitus in both lungs. In this case there were the ordinary febrile symptoms of the epidemic, and the characteristic prostration of strength and depression of spirits. Being anxious to exhibit to the pupils the effects of wine alone, we purposely withheld every other means, although we should have, no doubt, derived benefit from either sinapisms or blisters applied to the chest, as co-operating with the internal stimulant. But if we had applied these we should not have been able to prove, as conclusively as we expected we should be able to do from the use of the wine alone, that the pneumonia was a state of asthenic congestion, requiring a special modification of treatment. Under the use of wine alone the pulse became much less frequent, the skin much less congested, the lungs much more free, and all evidence of congestion, as evidenced by the presence of crepitus, disappeared. We adopted exactly the same treatment in many cases where delirium was present, and with the same beneficial results. How to deal with delirium in fever is perhaps the most difficult point in medicine, and the one which, more than any other, proves the skill and tact of the practical physician. Experience alone can teach it; and no one's experience will serve for another. The great difficulty of medicine consists in this, that exactly the same result will often depend upon the most opposite causes; and of course it is of the utmost impor-

tance to distinguish between these causes, as the treatment of the results must be as opposite and different as they are. Every practitioner knows on what different conditions and states of the system dropsy depends,—that it may depend on anemia or on plethora. Who would be absurd enough to apply to this symptom the same unvarying treatment, irrespective of its cause? Yet this irrational practice is pursued and recommended by those who urge that the cause of disease generally is of so mysterious a nature that it is in vain, in most cases, to look for it, and even if found it does not, in its practical influence, repay the trouble of the inquiry. As palpable an instance of the dependence of the same symptoms on different, or even opposite, conditions of an organ is afforded by delirium in fever, a symptom connected with the most opposite conditions of the brain. Generally the delirium that comes on in the first days of fever is the result of determination to the organ, and its treatment is generally understood; the treatment of that which comes on later, when the system is exhausted by disease, is also now better understood than it was formerly. It is now known to depend upon a condition of the brain somewhat similar to the state of this organ from hemorrhage or any other cause productive of general exhaustion, and experience has set its seal on the value of stimulants. But this epidemic presented the peculiarity of exhibiting this symptom at an early stage of the disease, under the aspect, and requiring a treatment suited to it when it occurs later.

"Our pathological notions upon the subject of inflammation have undergone, in latter years, very decided modification. The change to which I allude consists in the acknowledgment of the fact that the intensest inflammation may arise in the weakest state of the system, and find its remedy in restoring the tone of the system. I had frequent opportunities of bringing under the notice of the Pathological Society, at their meetings, specimens exhibiting appearances of vascularity, such as

we had been accustomed to regard as characteristic of high inflammation, and which we knew must have taken place when the body was under the greatest weakness. I shall only adduce one case in illustration. A woman aged 48 came under my care, for general dropsy and oppressed breathing, which stethoscopic examination proved to depend upon effusion into the left cavity of the pleura. Under treatment her condition improved for a time, but the improvement was but transient. Her state soon became worse than before; her breathing became more oppressed than ever; she could no longer lie down in bed. Percussion and auscultation indicated a great extent of effusion into the left cavity of the pleura. Her aspect and the low type of her symptoms made me suspect the presence of some malignant disease. We thought it possible that the left lung might be the seat of this disease; but this could only be conjecture, and did not admit of any proof, as the extent of the effusion so separated the lung from the side that its condition could not be ascertained by the stethoscope or otherwise. It was the analogy of other cases that suggested to me the suspicion of malignant disease. Examination after death exhibited extensive sero-fibrinous effusion into the cavity of the pleura. A thick covering of lymph, deeply stained with blood, and of a bright florid colour, coated the entire pleura covering the left lung, especially internally, where it had contracted an adhesion with the pericardium. There was moderate effusion into the right pleural cavity, its pleural investment was also coated with lymph, exhibiting exactly the same appearance of blood in it(*a*); and in the portion that dips into the sulcus separating the upper and middle lobes, the false membrane contained small miliary tubercles. There were small crude tubercles in the top of the right lung, in the midst of a deeply congested condition of the pulmonary parenchyma.

(*a*) "The appearance was such as would be regarded as hemorrhagic pleurisy."

"If we had only seen the *post mortem* appearances in this case, and had never seen the subject during life, and had been asked to form our judgment of the symptoms from the appearance presented by the pathological specimens, we should not have hesitated to express our conviction that there must have existed symptoms indicating high inflammation. Yet such was not the case. No symptom during life prepared us for the *post mortem* appearances. What are the inferences deducible from this case? They are these, that appearances indicating high inflammation, and involving the most important organs of the body, may develope themselves in the most damaged condition of the system, and by their absence of symptoms give no evidence of the existence of such inflammation. The low, depressed state of the system, which made us suspect malignant disease, was attested by the development of tubercles both in the lungs, in the midst of pulmonic structure in the condition that we regard as the first stage of pneumonia; and in the midst of the false membrane, an unequivocal result of inflammation. And if, at the same time that these low inflammations existed, there was another whose anatomical characters alone seemed to intimate a higher grade of disease, we cannot but believe that our notions of disease drawn from anatomic characters solely must be in some cases deceptive, and cannot be safely trusted as the grounds of treatment. We have already digressed more perhaps than we ought from our original subject, seduced by a point of great pathological and practical interest, and not yet sufficiently appreciated.

"We revert to our treatment of the fever by stimulants. It was a constant remark of M. Gueneau de Mussy, who attended my visit with a zeal and diligence that would have done credit to the most ardent student, and whose life had well nigh been forfeited in this hazardous service, *Si l'on avoit ces malades dans les hopitaux à Paris on les saigneroit, mais vous leur donnera du vin, et on se guerira.* At this time the proportion of our mortality was small beyond all precedent, not amounting to

two per cent. Nor was this because the fever was a mild one, for such was not the case, as no fever attended with such complete prostration of strength could be considered mild, but because no vital organ was involved, and therefore no obstacle existed to our employing the means that the primary fever required.

"We never encountered an epidemic fever in which there were fewer cases of cerebral excitement; yet there were some cases, and some of these were cases which, while they required stimulants, could not bear wine. To these I gave tartar emetic in cardiac mixture, sometimes alone, sometimes combined with opium, and always with decided benefit. To the good effects of the combination of tartar emetic with opium I have already adverted in my essay on fever, in the twelfth volume of the first series of the Dublin Journal; and in a lecture published in the eighteenth volume of the London Medical Gazette, in 1836, I have proved my claim to having been the first to use it in the treatment of delirium tremens; and if any benefit has resulted from its more extended application to other pathological conditions, I must arrogate to myself the merit that has accrued to medicine from its more extended use.

"As in former epidemics, the fever seemed to be modified by the rank and condition of the patient. Those in better circumstances generally exhibited a worse type and character of disease. There is in Dun's Hospital a pay ward, for admission into which a small sum is charged. This ward is generally occupied by a class somewhat above the common run of patients; in it the worst cases of fever were generally found. They ran a longer and more protracted course. The stimulant did not produce the same effect on them as it did on the poor patients, probably because they had been already habituated to it.

> 'Tibi quidnam accedet ad istam,
> Quam validus præsumis mollitiem.'

"We have already remarked that there were very few

cases of complete crisis, but several cases where there were as many as three or four imperfect crises, the disease at length disappearing. It seemed as if the energies of the system were unable by one effort to fling off the disease, and must needs by repeated attempts accomplish what it failed to do in a single one; and that this should be the case we can easily understand from the extreme prostration that marked the disease.

"We ought to have noticed amongst the symptoms one which occurred rather frequently; we allude to distressing irritability of the stomach. In some of these cases the discharge of a quantity of bitter fluid from the stomach appeared to act as a critical discharge, being followed by a marked mitigation of all the febrile symptoms, the leading one of which had been what the French so significantly describe as *embarras gastrique*. The remedies we found to relieve this irritability were,—ice, a blister sprinkled with morphia applied to the epigastrium, and a mixture containing five ounces and a half of infusion of mint, one drachm of liquor of potash, twenty drops of sedative liquor of opium, and three drachms of tincture of orange peel. One or other of these means generally succeeded in relieving this often most distressing symptom.

"From the frequency of relapse, we soon learned the prudence of keeping our patients in hospital till their convalescence was quite established, which in many cases was no easy matter. In most instances, where they left before we deemed it advisable that they should leave, they returned, often deeply lamenting their waywardness and imprudence, the second attack being sometimes, although not generally, more severe than the first. Two relapse cases exhibited to us the most striking specimens of those pseudo-rheumatic pains to which we have already so often alluded. One case far exceeded the other in its duration, and also in the symptoms exhibited during its protracted course. It was the case of Mary M'Dermott, aged 25, a servant. She was altogether ninety-two days in hospital. For a considerable time her illness consisted of

a kind of intermittent cerebro-spinal meningitis, a regular succession of attacks assuming a variety of form, and exhibiting various symptoms at different times. At one time she complained of most distressing pain in the back of the neck, extending along the arms even to the fingers. In another attack she lost the power of articulation, and her memory seemed to fail. We successfully combated these attacks with leeches, and mercury exhibited so as to touch the gums. Many of the symptoms present in this individual, and in others, strongly reminded us of what we had observed in cases generally regarded as hysteria, and therefore looked upon as disease "*nullius certæ sedis*," but to which our increased pathological knowledge, especially of the nervous system, has enabled us to assign a fixed locality. To this also we have been helped by the results of the application of remedies. The practical physician knows well the value of mercury in positive cerebral disease. We have already, in the former Series of your Journal, recorded our experience on this subject—an experience which our practice has confirmed. If we find that the disease which we call hysteria exhibits itself by symptoms indicating an affection or implication of a portion of the nervous system as palpably and unequivocally as those symptoms which we have no hesitation in referring to an actual disease of the brain, and if we apply to the so-called hysterical affection the same remedy or remedies that we have successfully applied to the cerebral affection, and with the same successful result, does it not at least argue a strong presumption that there exists a close relation, if not an actual identity of nature, between the pathological conditions of the two portions of the nervous system, giving rise to similar symptoms and amenable to the same treatment; and that, if there be actual disease of the brain, there is also actual disease of the portion of the nervous system whose derangement constitutes what we call hysteria? We adduce the following very interesting case in proof of our remarks:—Eliza Warren, aged 26, of an irritable temperament, had been often under our care

in hospital for hysteria, under various modifications and forms. At one time, pain under the left clavicle, with comparative dulness on percussion, and feeble respiration, with hæmoptysis, excited the suspicion of incipient phthisis pulmonalis. At another time she had uterine hemorrhage so profuse and so prolonged that it was supposed there must be some organic disease of the uterus. The disappearance of these two suspicious affections, and their place being taken by others of a more distinctly nervous character, explained in some degree their nature. On one occasion we received her into the hospital in a state of great nervous excitement, caused by her being rudely laid hold of by a soldier in the street, as she was returning home late in the evening. We had often seen her in nearly as great a state of excitement before, and relieved her with camphor and fetid spirit of ammonia. However, her present attack proved less tractable than preceding ones, and a new feature soon exhibited itself. At a late hour of the night of her admission into hospital, she awoke, and attempted to speak to the patient that lay in the bed beside her, but to her utter dismay she found she could not articulate. When we saw her the next day, we found that she could not utter a word; she could only make signs. The tongue was firmly pressed against the roof of the mouth, and she gave us to understand by signs that she felt pain proceeding from the back of the head to the tongue, which we concluded to be in the course of the lingual nerve.

"Connecting this strange symptom with others that she presented at the same time, and regarding them all as nervous, we employed the usual medicines exhibited in aggravated hysteria, such as camphor, valerian, the valerianate of zinc, castor, &c., but without the least benefit to our patient. These means having completely failed, we now gave her mercury, and with six grains of calomel in divided doses, in two days, her mouth became very sore. Immediately her tongue was loosed and she could speak quite plainly; her sore mouth was the only

impediment to her distinct articulation. After the salivation, which was profuse, had ceased, we gave her tonics, and in a short time she left the hospital quite well, and soon went to service. The immediate recovery of speech on the mouth becoming sore was conclusive to us that the one was the effect of the other. But, it might be objected, is it safe to rest a conclusion upon a single case? might it not have been a *post hoc* rather than a *propter hoc?* We had the opportunity of answering such objection by evidence that could not be resisted, the subject of it being the same individual, who, while in service, had a return of her illness, with the same utter inability to pronounce an articulate sound. The family in whose service she was, and who knew nothing of her previous illness, sent her into another hospital, where she continued for seven months, unable to utter a word. During this time she underwent a great variety of treatment, but with no beneficial result. She at length left the hospital by stealth, and came into Sir Patrick's Dun's Hospital, under our care. She was affected exactly in the same way as she was when she was under my care before. I, of course, at once resorted to the mercury, and ordered for her five grains of hydragyrum cum cretâ thrice daily. On the third day her mouth had become sore, and on the same day her speech was restored.

"This case, not less remarkable for its twice exhibiting the unusual phenomenon of loss of speech than for the immediate disappearance of this phenomenon on the system becoming affected with mercury, and that, too, after the failure of all those remedies that are usually employed in what are designated nervous disorders, has led me to look for some positive and organic lesion as not unfrequently lying at the root of these so-called nervous diseases.

"Every practitioner who witnessed the cases of cerebro-spinal meningitis was struck with the similarity of symptoms exhibited between this affection and hysteria. And as in this latter affection the symptoms sometimes exhibit themselves

only in derangement of the functions of organs that derive their nerves from the ganglionic system, while at other times they exhibit themselves equally in disturbance of the cerebro-spinal system of nerves, so was it with cerebro-spinal meningitis the disease seemed equally to involve both the ganglionic and cerebro-spinal nerves.

"Of the cases that came under my notice, two exhibited as marked opisthotonos as in tetanus, while they also presented symptoms intimating disturbance of the ganglionic nervous system. Their symptoms were exceedingly like those of cholera, and the messenger who was sent to fetch me to see one of them told me he was taking me to a case of cholera, although the disease did not prevail at this time, nor had there been a single case of it in Dublin for years.

"Dr. Darby, of Bray, who was the first to notice cerebro-spinal meningitis in this country (for the disease was first observed in France, where it prevailed as an epidemic), seeing a disease more like cholera than any other disease he had before met with, at first thought it was cholera, until ulterior experience of it proved it to be different.

"The cases of fever which constitute the subjects of the foregoing observations, for the most part, came from the rural districts of Donnybrook and Castleknock, the Guardians of these districts having made arrangements with the Governors of Sir Patrick Dun's Hospital to receive them. From June 28, 1848, the wards were opened to receive these patients, and from this date to August 31st, 160 patients were received and treated, without a single death. In September, a most intractable dysentery exhibited itself, and proved fatal in many instances. It did not occur as a complication of the fever, but as pure dysentery. In all the fatal cases *post mortem* examination exhibited extensive ulceration and sloughing of the mucous membrane of the large intestines, and in parts large deposits of lymph on what to the superficial observer would seem to be the mucous membrane. But a careful exa-

mination satisfied us that wherever the lymph was to be found the subjacent mucous membrane had disappeared, and that the lymph was not the product of the mucous membrane, but of the submucous cellular tissue. We had already, from previous examinations of individuals who had died of fever complicated with follicular enteritis, learned to question what had been received as a pathological fact, that where lymph was found it was the inflamed mucous membrane retaining its integrity that threw it out. On removing the lymph in these cases, we always found that the membrane underneath exhibited none of the smooth villous appearance that naturally belongs to mucous membrane, but resembled a cellular or serous membrane; that it was in fact the cellular membrane brought into view from its investing mucous covering being destroyed. Finding this to be the case in fever complicated with enteritis, we naturally expected and found it to be the same in the case of pure dysentery. From this we doubt not that it is a pathological error to attribute the effusion of lymph in dysentery to the mucous membrane, and regard it as the product of the cellular membrane corresponding to the deposit.

"We conceive it also highly probable that this is the explanation of Hunter's experiment of causing the adhesion of the opposite surfaces of the vagina of the ass, by an injection of a strong solution of corrosive sublimate, an experiment so often referred to in proof that mucous membranes only require a certain degree of inflammation to cause them to throw out lymph, and then to adhere. We believe the strong injection destroyed the mucous membrane, and, inflaming the subjacent cellular membrane, made it throw out the lymph. Besides the plausibility of our explanation, we cannot suppose that mucous membranes, occupying the position that they do in the economy, namely, lining canals whose permanently open or patent state involves such serious consequences, should be susceptible of adhesion under almost any degree of inflammation. We should expect that they would die first, which we believe to be the

fact(*a*). Amongst the cases of dysentery was one occurring in a sailor, which presented a very unusual complication. He was labouring under the disease in a most aggravated form, when the wrists and knees became painful and swollen, and exhibited the suspicious aspect of purulent arthritis. The irritative fever under which he laboured at the time strengthened my suspicions as to the nature of the arthritic affection. The case, contrary to our expectations, terminated favourably. We treated it with calomel and opium, and blisters and stupes applied to the abdomen, and leeches to the verge of the anus(*b*), while tincture of iodine was applied to the inflamed joints. During the months of September and October, and to the 8th of November, we received into hospital 270 cases of fever, eight of which terminated fatally. The fever now assumed a worse type than it had hitherto presented. Amongst these fatal cases there were six males and but two females. The wards of the hospital, according to the arrangements to which we have above referred, were

(*a*) When I stated to Dr. Carte, Curator of the Museum of the College of Surgeons, my view both as to the lymph supposed to be deposited on inflamed mucous membrane, and also my explanation of Hunter's experiment, I was gratified to learn that this opinion on both these points was that held by the late Professor of Anatomy in the University, Dr. Macartney, whose pathological views were singularly exact, and, on many points, much in advance of the pathology of his day.

(*b*) The beneficial effects that we have seen experienced from leeches applied to the verge of the anus, both in acute inflammation and chronic congestion of the intestinal mucous membrane, induce us to press the point strongly on the attention of the physician. To no part externally can we apply leeches where they could possibly exert a more direct or immediate influence on unloading the congested vessels. And I am satisfied that were this locality oftener chosen where to apply leeches in hepatic affections, we should more rarely have dropsy or enlarged spleens as the result of such affections. Are we quite sure that nature does not, at least sometimes, establish hemorrhoids for the purpose of relieving the system? That she does make use of them when already existing, for this purpose, is beyond all question. Ought we not then take the hint from her, and let art adopt the suggestion of nature? Continental physicians know the value of this mode of applying leeches, and practise it most largely and beneficially.

opened on the 28th of June, and were closed, in consequence of the epidemic abating, on the 8th of the following November. During that time 336 fever patients were treated. Amongst these there were but eight deaths, an unusually small proportion of mortality; little more than two per cent. To what are we to attribute the small mortality? Is it to the mild character of the fever? While we do ascribe to the character of the fever a mortality small beyond all precedent, we conceive it would be to misrepresent it to call it a mild fever. The duration of a fever is, to a certain extent, a measure of its ordinary severity. The milder fevers in general run their course in a shorter time than those of a more severe type. Of these latter the fever seldom exceeds three weeks, unless it be kept up by some complication which prolongs its normal duration, and in general convalescence is complete in a month or five weeks. Applying this rule to the cases that we treated, we find that out of the 336 cases seventy-two remained in hospital more than thirty days; that eighty-two remained more than twenty days, and ninety-three more than ten days. According to this I conceive we can hardly call it a mild fever. To the peculiarity of its character, and to the simplicity of its treatment, we attribute its small mortality. The peculiarity to which we especially allude is the freedom from any complication of any organ or apparatus calculated to give it a dangerous character. As to the treatment, we never met with an epidemic where there was so little room for mistake. Stimulants alone were required, and were almost universally borne by the patients. They never seemed to be over stimulated. Of the 336 cases there were 148 females and 188 males.

"As to the contagious nature of the disease, we infer it from the fact that we have had in hospital, at the same time, entire families, embracing all ages.

"The striking peculiarities of this fever, as compared with preceding epidemics, seemed to us to consist in the complete absence of any complication except where the bastard rheumatism

indicated an affection of the spinal nerves, and which alone excited alarm when affecting the top of the spine and back of the head.

" Secondly,—In the extreme and overpowering sense of prostration of strength and depression of spirits, which imparted to it much of the character of aggravated influenza.

" Thirdly,—In exhibiting something like a crisis, but seldom complete, yet more of true crisis than the fever of recent date.

" Fourthly,—In the great tendency to relapse.

" Fifthly,—In the almost universal tolerance of stimulants.

" Sixthly,—In the absence of petechiæ.

" Seventhly,—In the absence of bed-sores.

" Eighthly,—In the tolerance of any amount of local bleeding, and the intolerance of the smallest amount of general bleeding.

" Ninthly,—In presenting some features of resemblance to ague by involving the spleen, and, like it, not affecting the sensorium."

[This Report upon the recent epidemic fever in Ireland having, in our last four Numbers, occupied so much of the space usually devoted to original communications in our Journal, that we have been obliged to exclude several valuable papers during the past year, we have determined to bring it to a conclusion in the present Number, and to print the General Summary in the separate publication of the Report, which will be published forthwith.—Ed.]

Art. VII.—*Contributions to the Pathology and Treatment of Eruptive Diseases of the Skin.* By J. Moore Neligan, M. D., M. R. I. A., Physician to Jervis-street Hospital, and Lecturer on the Practice of Medicine in the Dublin School of Medicine.

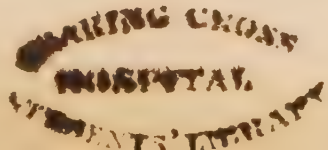
I purpose, in a series of short essays, to bring before the profession some facts regarding cutaneous diseases, which, in the course of my experience, have forced themselves on my attention; and, at the same time, to offer some observations that have been derived from their treatment, in both hospital and private practice.

Of the various affections of the skin, there are none which more obstinately resist remedial measures than the scaly eruptions. Constituted by Willan into a distinct order, and divided by him into four classes, lepra, psoriasis, pityriasis, and ichthyosis, this arrangement has been generally adopted; and although the necessity or practical advantage of any subdivision of eruptions distinctly characterized by the peculiar epidermic secretion of scales may be doubted, I am very unwilling, as I have already stated in a previous communication in this Journal, to complicate the study of an already difficult subject in practical medicine, by any innovations on what may be termed established principles. Mr. Erasmus Wilson, in his beautiful pictorial work, and also in his volume on diseases of the skin, removes from Willan's order of squamous eruptions, pityriasis versicolor, and pityriasis nigra, adopting Rayer's nomenclature of chloasma and melasma, and regards them as *alterations* in the colour of the skin, accompanied by a foliaceous or mealy desquamation, and not by the separation of true scales. As, however, in both of these forms there is a squamous secretion, differing only in degree from the more distinctive separation of scales in the other classes of the order, and as there does not appear to be any essential difference in their treatment, I pre-

fer to regard them, with Willan, as varieties of pityriasis. Icthyosis must, I think, be removed from the order, for in it there is no separation—throwing off of the scales, or desquamation, which I look on as the most characteristic sign of this group of cutaneous eruptions; and I would furthermore simplify the classification, by reducing the classes to two, namely, psoriasis, under which appellation I would include the various genera of lepra, and pityriasis.

Probably the greatest difficulty in the study of cutaneous eruptions has arisen from the multiplicity of subdivisions into which they have been arranged by successive authors. Each writer endeavouring to improve on those that preceded him, instead of attempting to simplify the subject, and make his classification subservient, not alone to the diagnosis but to the treatment of these affections, by reducing the classes, orders, and genera, to as few a number as possible, has, in most instances, proposed either to alter completely existing classifications, and to substitute an altogether new nomenclature based on some theoretical notions of his own, or to make numerous additions to, and minute subdivisions of the arrangements of others. The student of these diseases is thus much perplexed, and too frequently abandons the subject in despair. In truth, the diagnosis of eruptions of the skin, so far as regards the distinction of one class, or of one order, from another, is simple enough, and it is only when we come to the definition of the genera, that much difficulty arises. What practical advantage, moreover, is to be gained by these minute subdivisions? The general principles of treatment adapted for the class are equally applicable to the genera; and I cannot admit the propriety of artificially classifying any set of diseases, solely with regard to their *appearances*.

Psoriasis, and its variety lepra, are so well described in all works on diseases of the skin, and their characteristic features are so defined, as not to require any further observations on their diagnosis.



The causation of these affections is, like that of all cutaneous eruptions, enveloped in much obscurity. That they are sometimes produced by the action of certain irritating matters on the skin cannot be doubted; yet we see the same local forms of psoriasis arise in persons who, from their occupation or position in life, are not exposed to the causes which produce it in bakers, washerwomen, shoemakers, and individuals of other trades. That they are sometimes hereditary is also true, but their hereditary nature is of a singular character; thus, so far as my own experience would lead me to infer, the disease does not descend directly from parent to child with the same regularity as other hereditary diseases, but the predisposition frequently appears to be derived from an uncle or aunt, whose own children may be free from the disease; and it also seems not uncommonly to lie dormant in a family for a generation, and again re-appear. In a case which I not long since attended, one son of a family was affected, from his childhood, with psoriasis of an inveterate form; none of his brothers or sisters had any cutaneous eruptions, nor had his parents or grand-parents, but a paternal uncle was affected with a similar disease, and also a first cousin, not the child of this uncle: it was also stated to me that a grand-uncle was a sufferer for years from a cutaneous eruption. A somewhat similar hereditary transmission of the disease has fallen under my notice in several instances, namely, that while the immediate descendants have escaped, one or more members of collateral branches have been affected. The true hereditary nature of psoriaris and lepra might, therefore, be doubted, and the cases which occur, and may be adduced as proofs of it, might be accounted for in the same manner as those in which no such origin can be traced, and which are usually stated to depend on some constitutional peculiarity, not better understood now than in the days of Willan, who described it as characterized by "a slow pulse, or a languid circulation of the blood, and, what must be generally connected with it, a harsh, dry, impermeable state of the skin and cuticle." The

latter part of this observation has been adopted by Mr. Erasmus Wilson, who says that "the disease appears for the most part in those who are remarkable for a dryness of the skin." Yet I cannot help thinking that this is a confounding of the effect with the cause, and that it would be rather hazardous to predicate the likelihood of psoriasis occurring in an individual because his skin was unusually dry or harsh.

Before I proceed to speak of the treatment of psoriasis and lepra, I wish especially to impress upon the profession the chronic character of all squamous eruptions; not that I consider the fact as already unknown or insufficiently known to all, even the youngest practitioner, but that I think it is too often overlooked in the prognosis given to patients or their friends as regards the curability of the disease, and especially as to the time required to effect a cure. Developed under the influence of a peculiar constitutional state of the skin, essentially of an obstinate character, and most apt to re-appear even in months or years after they seem to have been completely cured, length of time is as essential to their perfect removal as the most judiciously planned course of treatment. The physician should, therefore, in every case, be most careful not to promise a speedy cure, and always, before prescribing, explain to his patient the chronic character of the disease, and that it requires a steady perseverance in the use of remedial measures for at least two or three months before even an apparent amendment will be perceptible. The anxiety of mind which an individual labouring under a cutaneous eruption suffers is very great, and this too adds to the difficulty of treatment. The promise of an eventual cure, though after a lengthened period, tends to alleviate this anxiety, and prevents the repeated disappointment, changes of medical advisers, and trials of new plans of treatment, which the hope deferred, when a speedy cure has been promised, causes.

"It is an admitted fact," writes Cazenave, "that the therapeutics of these diseases rests upon purely empirical grounds,

and that, unhappily, there exists no sure guide to direct to a rational mode of cure." This statement, sufficiently true of most eruptions of the skin, is equally so of many other diseases of the body, and it should teach us not to despise the light thrown on pathology by the experience derived from therapeutics. When it is found that a certain class of remedies acts beneficially on deranged conditions of the animal economy, concerning the true nature of which doubt exists, it cannot be termed a *petitio principis* to infer that such derangements have a similarity in a greater or less degree to affections the nature of which is known, and which are benefited by the same class of remedies. In the treatment of scaly diseases of the skin I have found iodine, in some of its various combinations, especially useful; and I would, even from this therapeutic fact alone, be inclined to look upon the peculiarity of constitution in which they occur as nearly allied to the scrofulous; in fact, that their appearance is but one of the protean forms in which scrofula may be developed. And, independently of the beneficial effects of iodine, if we look to the remedies ordinarily proposed as specifics for their cure,—of course I speak only of those administered internally, or so to say, constitutionally,—what are they but tonics, alteratives, or diaphoretics, generally employed in the treatment of scrofulous affections? Again, if we lay aside the analogy derived from therapeutics, in how many points do not scrofula and scaly eruptions of the skin agree? —their hereditary nature, their slow development, the period of life at which they appear, their production by innutrition or mal-innervation of the system, their obstinacy, their liability to recur or be again reproduced, the diathesis of the individuals in whom they appear, &c.

In the treatment of psoriasis in any of its forms, amongst which I include the various genera of lepra, the internal remedies chiefly employed formerly were those which were known to act as diaphoretics; and which were given with the view of producing an alteration in the secreting apparatus of the skin.

This may be called a *rational* plan of treatment, it being applied to remove the condition on which the disease was supposed to depend, namely, defective cuticular secretion. But this class of medicines, although in most cases affording relief to symptoms, was not found to cure the disease; and, consequently, various remedies were used *empirically*, and the so-called specifics began to be employed. Of all these none appeared to effect a cure so frequently as arsenic, whether given alone, or its administration conjoined with the application of various local agents; and this medicine still holds its sway as the best specific for scaly eruptions, but it is now generally given in some combination with iodine.

The remedial efficacy of arsenic in these diseases I do not mean to dispute; indeed my experience would not permit me to do so: but I wish to show that in every case its beneficial action is more decided and more speedily manifested when the administration of iodine, or of the iodide of potassium, is employed at the same time, or alternated with it; and also that in those cases—and they are not a few—in which arsenic, no matter how prescribed, disagrees, the latter medicine alone is generally productive of the best effects. The same may be also stated of those cases in which, from any cause, the patient objects to take preparations of arsenic.

With the view of exhibiting the good effects of iodide of potassium combined with diaphoretics, I annex the particulars of one of the most inveterate cases of psoriasis that I have ever seen. I give the report of the case as described in a letter I recently received from the gentleman himself, a talented member of one of our learned professions. It presents a most graphic account of the disease.

"You wish for a detail of the disease for which you have so successfully treated me.

"Since my boyhood I have had patches of psoriasis on various parts of my body. They first appeared on the right leg, and soon after on the left. The treatment then employed

was some preparation of tar, in an ointment, applied to the parts affected, sarsaparilla, and some pills containing a small quantity of mercury. I did not persevere in the use of these remedies. The eruption was not painful or troublesome, and for several years I did nothing whatever in the way of cure.

"In the spring and autumn, it used to extend itself to the arms, the inside of the thighs, and about the hips, seeming to select the vicinity of joints. But at the other seasons of the year, it retreated to its old lodgment, the calves of the legs.

"About six years since it became extremely troublesome and painful. From having been indolent, it was inflamed and irritated, so much so as to produce sympathetic swellings in the groin. I then, under medical advice, applied poultices to lower the inflammation, and remove the scales; dressed the sores with a mild mercurial ointment, and as an internal remedy took some pills, of which Plummer's Pill was the chief ingredient. I persevered in this plan for about three months. It effaced the disease, but did not extirpate it, for the moment I discontinued the remedies, the ailment returned. After a while I tried liquor arsenicalis, but, thinking it a dangerous remedy, from its accumulative properties, I did not persevere long enough to judge fairly of its effects.

"In the autumn of 1848 the sores on the legs became very painful, so much so as to render walking inconvenient. I was advised to swathe the lower limbs in wet lint, kept moist by an oiled silk covering, and secured by a stocking bandage.

"From this mechanical treatment I derived great local relief. As a constitutional remedy I took liquor potassæ three times a day in a little milk, and paid some attention to diet.

"Unquestionably the system did me good, but I cannot say that the character of the sores changed much, and I soon abandoned it. It was, I think, in January last, that I consulted you.

"The limbs were inflamed, and the skin tense. In the neighbourhood of the affected parts there were small sinuses

in which matter was formed. The sores when touched bled freely, and they were very angry and ill-conditioned in their appearance. I persevered in your treatment for three months, and derived the greatest benefit from it. Under the influence of the tepid bath, and the alterative remedy prescribed by you, the disease has vanished from my body, save from the calves of the legs; but there the character of the affection has materially altered. Sound portions of skin have insulated themselves in the midst of what was a mass of leprous irritation, troublesome to myself, and unsightly in the extreme.

"I bandage the parts with old linen, take a warm bath at about 92° every week, and continue the use of your medicine. Mine is a very inveterate case, of more than twenty years' standing. I have tried several remedies, but the only two that were fairly persevered in are the mercurial and your's.

"I will not, therefore, comment on the others, as I cannot say what they might have done; but of the two above alluded to I prefer your's, because I think the effect more permanent, and purchased at a less risk.

"If mercury had driven the disease out of the constitution, I would have been cured, but it did not. Your plan of treatment, mild and simple, will always keep the disease down without the unpleasant necessity of having recourse to mercurials."

The plan of treatment which I prescribed for this gentleman consisted in the administration of five grains of iodide of potassium, dissolved in two ounces of a decoction of the *fresh* inner bark of the elm, taken every night at bed-time, and the employment of a fresh-water bath at the temperature of 92° F., at first three times, and afterwards twice weekly. In his case it is probable that the arsenical preparations might have acted equally beneficially, but he had an insuperable objection to take arsenic in any form; and from my previous experience of the good effects of the iodide of potassium in many cases where arsenic failed to cure the disease, I

confidently adopted its use, and the result has proved that I did not overrate it as a remedy for inveterate psoriasis.

Donovan's solution of the hydriodate of arsenic and mercury has been extensively employed in the treatment of this class of cutaneous eruptions, and the pages of the former series of this Journal bear ample testimony to its good effects. I must, however, confess that I am not partial to the employment of any mercurial preparation in their treatment, when they affect the body generally; and the only local form of scaly diseases, in which I have found a mercurial of use is in pityriasis of the scalp, as I mentioned in a former essay, and in it I still find the yellow iodide of mercury productive of the best results.

The iodide of arsenic has also been found a remedy of decided efficacy in the treatment of these affections; it was first generally employed for them by the late Dr. Anthony Todd Thomson, of London, and the testimony of those who have used it is confirmatory of his observations. My own experience leads me to place more reliance on it than on any other preparation of arsenic; but, although I have found it, when administered alone, capable of curing many case of psoriasis and lepra, I consider that its beneficial action is much augmented by combining it with the iodide of potassium, and with iodine in the form I shall hereafter describe. The following case is selected from many, as a proof of the effects of this remedy in producing a permanent cure. It was reported by Dr. T. W. Christie, now of Sidmouth.

Daniel Callaghan, nineteen years of age, a baker by trade, of middle stature and sanguine temperament, was admitted into Jervis-street Hospital, February 7, 1843, affected with psoriasis inveterata. The greater part of his body, and particularly his legs and arms, is covered with an eruption consisting, in some places, of small circular scales, and in others, of large irregular patches coated with white scales, and having an elevated border. There is an abundant desquamation of the cuticle, especially from the lower extremities. The scalp is also affected,

but the spots there seem to be more circumscribed. The tongue is rather clean; the bowels constipated; the appetite good, but he states that his strength is latterly much diminished.

From his own account, he was always healthy previously to his becoming a baker (about five years since); eighteen months subsequently to which period, having lived intemperately while exposed to the great alternations of temperature inseparable from his trade, his present disease made its appearance. It came on in the form of minute, red, elevated spots, about the size of a small pea, extremely itchy, and rendered more so by heat; they did not exude any moisture except when severely scratched; scales soon formed on them, which continually fell off, but to be renewed by others; and, rapidly spreading, they soon covered the whole of his body, except the face and hands, which have but partially suffered. He has been treated by various medical men during the last three years, but he has never received more than partial benefit from the many remedies tried with him. On admission he was ordered a warm bath and purgative medicine, and the next day a twelfth of a grain of the iodide of arsenic, made into a pill with liquorice powder, was prescribed for him, to be taken three times daily. This treatment was continued, the dose of the iodide of arsenic having been gradually increased to a sixth of a grain three times daily, until the 3rd of May, when he was discharged from hospital without a trace of the disease remaining. The medicine had occasionally to be omitted for a few days, when the system gave evidence of being saturated with it, and the symptoms then produced were readily removed by an active saline purgative, and the omission of the iodide, generally for two or three days.

This patient, during the entire time that he was in hospital, was kept *strictly on milk diet.*

This case is a good example of those in which the iodide of arsenic alone effects a rapid and permanent cure; but unfortunately, in most instances, it does not produce such decided

beneficial results; and when, after a fair trial of it, I find that the disease is not disappearing, I generally combine it with iodine and the iodide of potassium, or, omitting it altogether, give the latter medicine in the decoction of elm bark; and in very obstinate cases I administer them alternately, usually every second fortnight.

The importance of a milk diet I have insisted on in my "Essay on Eruptive Diseases of the Scalp," and experience leads me to the conclusion, that an adherence to it is of the utmost consequence in cutaneous eruptions, except when they occur in cachectic or broken down-constitutions, or are attended with deep ulceration, or very profuse and weakening discharges. It is, of course, much easier to carry out dietetic rules with hospital patients than with those in private or in dispensary practice, and consequently more satisfactory results are generally obtained in treating the former; but if the necessity for their observance be strictly inculcated, and if they are laid down in writing as constituting an essential part of the treatment, I have, in most cases, found them to be pretty closely observed. When, however, any serious objections are raised to the adoption of a strictly milk diet, farinaceous articles and milk should be made, as much as possible, a portion of the general food, and, in addition, fresh meat, plainly dressed, or poultry, should be alone allowed. The patient, or his friends, should, at the same time, be informed, that the cure will be more tedious, and relapse more likely to occur.

Tepid baths of *fresh* water, twice or three times weekly, will be found an adjunct of much importance in the treatment of scaly eruptions; they tend to restore the natural secretions of the skin, and prevent the accumulation of scales. They are especially useful when much local irritation exists, particularly in those cases where deep bleeding fissures are present. I have tried various other local applications, such as ointments or lotions of iodide of sulphur, of iodide of potassium, of sulphur, of sulphuret of potash, &c., but my experience leads me

to prefer the plain tepid bath. Alkaline baths of soda are not, I think, so applicable to psoriasis or lepra as to pityriasis.

Chronic cases of psoriasis are very frequently complicated with derangement of the digestive organs, evidenced by various dyspeptic symptoms; the most prominent of these are nausea and vomiting immediately after meals. This condition must be remedied by appropriate alterative and tonic treatment, previously to the employment of medicines with the view of acting directly on the eruption, for if arsenic or iodine, in any form, be given while this condition is present, they will tend to increase the existing irritation, and their expected beneficial action be thereby prevented.

I have already stated that I have derived excellent effects in the treatment of squamous eruptions from a combination of the iodide of potassium with arsenic and with iodine. Commencing the treatment, as I usually do, with the iodide of arsenic, where nothing forbids its use, after it has been continued for a month or six weeks, I order a mixture containing two drachms of the liquor arsenicalis, two drachms of simple syrup, five ounces and a half of distilled water, and half a drachm of iodide of potassium. Of this mixture, I prescribe, at first, a dessert-spoonful three times daily, in a glass of *eau sucrée*, directing it to be taken after meals. This quantity contains five drops of the arsenical solution, and a grain and a quarter of iodide of potassium. To it I sometimes, in very inveterate cases, or after it has been continued, in gradually increased doses, for some weeks, without decided effect being produced, add six grains of iodine,—a fourth of a grain to each dose. The mixture then, chemically speaking, is a solution of the ioduretted iodide of potassium and arsenic. When iodine is added in this proportion to the mixture, I use syrup undiluted as the menstruum, and then direct the dose to be taken in plain water. If a flavoured syrup be employed, the mixture is far from being disagreeable. After numerous trials made for me by Mr. Oldham, of Grafton-street, I find that

syrup of capillaire(*a*) most completely conceals the odour and taste of the iodine.

The only other preparations of arsenic that I have been in the habit of employing in the treatment of these eruptions, are the arseniate of ammonia and the arseniate of soda. Both are apt to disorder the stomach, the latter especially so; yet they are sometimes productive of good effects, particularly in the local forms of psoriasis, as in psoriasis palmaris. The arseniate of ammonia I always use first, and generally alternate with it the arseniate of soda. They are best given in solution. The formula I prescribe is somewhat similar to that proposed by Biett, consisting of one grain and a half of the arseniate of ammonia dissolved in three ounces of water, to which are added six drachms of spirit of cinnamon, or of compound spirit of aniseed. The dose of this mixture is one drachm, which contains a twentieth of a grain of the arseniate, and it is gradually increased to two or three drachms three times daily, beyond which dose I do not find it safe or useful to venture. It is best taken in a little plain water, and either during or after meals.

In addition to the use of internal remedies, I always employ external applications in the treatment of psoriasis palmaris, and of the other local forms of the eruption. Iodide of sulphur, in the form of ointment, I have found productive of the best effects, but it must be used very much diluted in the first instance, and the strength of the ointment gradually increased. Not more than eight grains to the ounce of wax

(*a*) This syrup is much too little used in medicine as a flavouring ingredient; its odour and taste are peculiarly agreeable, very much more so than those of most of the syrups contained in our national Pharmacopœias. It is officinal in the French Codex; and, singularly enough, the variety of capillaire, the Adiantum pedatum, from which it is chiefly prepared, is a native of Canada, from whence it is imported into France: there can, therefore, be no commercial reason why it should not be in general use with us. I hope that the framers of the new edition of the Dublin Pharmacopœia now in preparation will introduce it amongst the officinal syrups.

cerate should be prescribed in the commencement of the treatment, and the proportion should never be greater than twenty grains to the ounce. The parts affected should be washed, at least three times daily, with a weak solution of the carbonate of soda, half a drachm to the pint of distilled water, and well dried afterwards, previous to the renewed application of the ointment.

As regards *pityriasis*, when it affects the body generally I treat it, if of short standing, as a mildly inflammatory affection, prescribing gentle saline purgatives twice or three times a week; an alkaline bath,—four ounces of carbonate of soda in sufficient fresh water for an ordinary bath, at the temperature of 80° to 92° F. according to the season of the year,—every second day, and in strong constitutions an occasional bleeding, either general or by the application of half a dozen leeches to the verge of the anus. But when the disease becomes chronic, I use the same treatment as I have above described for psoriasis: it must be remembered that the alkaline bath is not so useful in the chronic as in the acute stage of the affection. The local forms of the disease are, I think, best treated on the plan I have proposed for the eruption when it affects the scalp. The chief additional observation my subsequent experience enables me to offer on the local disease is, that it is of a much more inflammatory character in persons from the age of 12 to 35 than in the very young or the old, and consequently that the use of the mildest local stimulant in them will often cause a marked aggravation of the symptoms, and is therefore scarcely, if at all, admissible in its treatment.

I have found pityriasis a more obstinate eruption to treat than psoriasis, not that the effects of remedies are less marked in causing its disappearance, but it is much more apt to return even in months after an apparent cure has been effected. Whatever plan of treatment, therefore, I find successful in removing the disease in any individual case, I direct to be continued for at least a month or six weeks after the scaly secretion has

ceased, and in every case the use of fresh water tepid baths should be directed twice or three times weekly for several months afterwards.

In the commencement of this paper, I have given my reasons for not agreeing with Mr. Wilson in removing pityriasis versicolor and pityriasis nigra from amongst the squamous eruptions of the skin, and classifying them with those affections characterized by lesion of colouring matter. They are, so far as my experience enables me to judge, amenable to the same remedial measures as the other forms of pityriasis, and the plan of treatment adopted by Mr. Wilson, in the case reported in his beautiful folio work, does not differ in principle from that which he uses in squamous eruptions. I believe pityriasis versicolor to be but a variety of pityriasis rubra; the latter occurring usually on those parts exposed to the action of the air, as on the face, neck, and hands, being especially well marked on the fingers; while the former, to quote the words of Willan, "chiefly affects the arms, breast, and abdomen."

In concluding, for the present, I have to add but one or two words with respect to the clothing which ought to be worn by persons suffering from, or who have been affected with any scaly disease of the skin. Every observer of these affections has remarked their liability to re-appear, or be aggravated, in autumn and in spring. This teaches the necessity of avoiding sudden changes of temperature, and of keeping the surface of the body of as uniform a warmth as possible. Linen or woollen garments should not be worn next the skin, as they are apt to irritate. The under-clothing should be either of silk or cotton, the former I prefer; as, however, its price places it out of the reach of the poorer classes, the latter must be used by them; but the necessity of wearing a silk or cotton inside vest, with sleeves, must be impressed on all.

Art. VIII.—*Cases of Calculi in the Kidneys, with Observations.* By Cathcart Lees, M. B., Physician to the Meath Hospital. With a Plate.

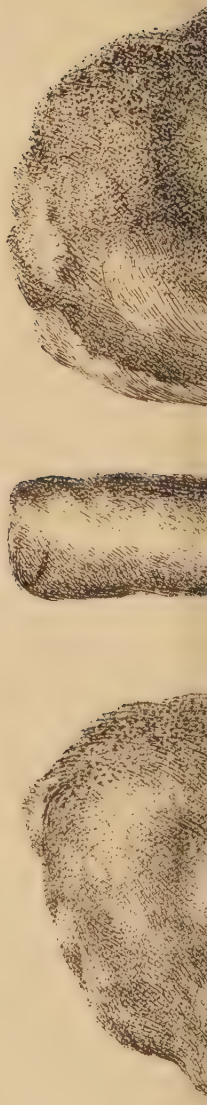
Although the present advanced state of pathology and chemistry has thrown much light on all the diseases of the kidneys, yet the practitioner must confess that he often feels embarrassed when called upon to decide as to the exact nature and cause of some of the frequent, though often obscure and painful symptoms, referred to these organs. It is, for instance, difficult to pronounce whether a patient's sufferings are caused by mere functional derangement, or are owing to some acute or subacute inflammatory attack of the substance or lining membrane of the kidney, requiring antiphlogistic treatment, but not likely to return if properly treated; or whether they are caused by the presence of a calculus in the kidney, or some degeneration of its substance malignant or otherwise, when the treatment must be chiefly palliative, and our prognosis, to say the least, very guarded as to the probable return and final result of such attacks. I therefore think it may be interesting to the profession, to give the details of two cases of calculi in the kidneys, where the symptoms were accurately observed by me during life, and the diagnosis confirmed by *post mortem* examination.

J. B——, aged 20, apprentice to a watchmaker, a strong lad, but of a sallow, unhealthy appearance, had been under my care occasionally for about two years, suffering from frequent desire to pass urine, with pain in the loins, particularly in the right side, sensation of nausea, and general uneasiness, but never so as to incapacitate him from attending to his business. The urine was always passed pale, turbid, and alkaline, a thick ropy mucus coming with the first gush; specific gravity 1·006; albuminous, and contained very little urea. The quantity of urine was scanty, but the deposit of ropy mucus very considerable. He was examined by Sir Philip Crampton and Mr. Read, who pro-

nounced that there was no disease of the bladder; the diagnosis was chronic suppuration of the kidney, with probably granular degeneration. His father told me that the lad had passed a small stone when he was about ten years old, but had the ropy mucus in the urine from infancy; and the same appearance had also been present in his mother's urine. He was relieved by treatment, but suffered from repeated returns of the complaint, till at last, after exposure to cold, he was attacked with severe pain across the loins, sickness of stomach, and great debility. I found him suffering from pyrosis, diarrhœa, dry parched tongue, constant nausea (no dysuria); pulse 100, very feeble and intermitting; countenance expressive of great suffering; the urine was pale, scanty, highly alkaline when passed; a copious deposit of ropy mucus, with a slight reddish tinge; there was great tenderness on pressure along both ureters. A consultation was held with Mr. Smyly, when we agreed that his sufferings were probably caused by a calculus impacted in the ureter. Two days afterwards, the abdomen being much retracted, I detected a hard tumour a little above the level of the umbilicus in the *right lumbar region*, anteriorly. The tumour was hard, tender, and could be moved laterally; he suffered constantly from severe nausea and retching, like the worst form of sea-sickness, and died suddenly, while being turned in bed; but, strange to say, he had not been confined to bed till within four or five days of his death.

The body was examined by Mr. Williams. Though it appeared greatly emaciated, there was a considerable amount of fat in the integuments of the abdomen. On putting aside the omentum and intestines, the tumour came into view, covered with fat, and lying close along the spine; it proved to be the right kidney, which was brought forward out of its proper place, and joined to the left kidney across the vertebral column, forming what is termed a horse-shoe kidney, the substance of one being continued into that of the other by a transverse portion passing across the spine. Both the sides and the trans-

Weight 1120 Grains



J. L. Draper del.

B. Keenan Lith. 4 Trinity St.

verse portion were filled with calculi. The pelvis of the right kidney was filled by a large calculus, which extended into the calices, forming branches, and which constituted the tumour felt during life; but both kidneys were quite filled with calculi of various sizes. The tubular structure of the kidneys was nearly obliterated, and the cortical substance much congested; the ureters were pervious, and slightly dilated; the bladder was of the natural size; no thickening of its walls, nor any columnar fibres, which was satisfactory in confirming the opinion as to the healthy state of that viscus, which had been previously given. It presented, however, a curious appearance, the mucous membrane being covered over with numerous vesicles, which contained a gelatinous fluid. The other viscera of the abdomen appeared healthy, as also the viscera of the thorax. The calculi, when removed from the kidney, presented, over the most of their surface, a rough, white, granular appearance, covered here and there with patches of a dark brown colour, composed of oxalate of lime; the chief bulk of the calculi was, however, composed of triple phosphate with phosphate of lime; of these the latter was by much the more abundant, as was proved by dissolving out the triple phosphate by acetic acid, and by the calculus fusing only slowly and partially before the blowpipe. The phosphate of lime, almost all of which resisted the action of the acetic acid, was instantly and completely dissolved on the addition of dilute muriatic acid. One of the largest calculi weighed 1120 grains, and is represented in the plate. I made sections of some of the calculi, but the composition was the same all through. He had never passed any sand in his urine, in which, when examined with the microscope, minute *acicular* prisms of the triple phosphate only were observed.

William Clarke, a car-driver, was admitted into the Meath Hospital for bronchitis. On going round the wards I observed this man vomiting, and on inquiring as to the cause, he said he vomited constantly, and attributed it to his cough. On investigating his case I found that he suffered from constant

dull pain in the right lumbar region, with severe pains in his feet, and vomited every morning, but passed urine without any annoyance. He stated, however, that he was formerly a soldier, and that in Africa, twenty years ago, he contracted fever, and at that period suffered from some urinary affection, having occasional retention, with severe pain in the loins and region of the bladder. He was discharged and gradually recovering, continued in good health till about six years since, when he was attacked with severe pain in the lumbar and pubic regions, accompanied by obstinate vomiting, which persisted for three days, when, after violent straining, he passed a small stone by the urethra. The urgent symptoms immediately subsided, and he continued free from suffering till about three months since, when the symptoms of which he now complains made their appearance. The urine was now found to be large in quantity, of a pale opaline colour, alkaline immediately after being passed, spec. gravity 1·007, albuminous; a copious deposit of white sediment subsides to the bottom of the vessel, while an iridescent pellicle floats on the surface; on submitting the urine to microscopic examination, large triangular prisms of the triple phosphate were seen, with amorphous phosphate of lime. I made the diagnosis of a calculus in the right kidney, and put him on a generous diet, with dilute nitric acid, and mild counter-irritation to the right lumbar region, under which treatment he was progressing favourably, when unfortunately he was attacked with erysipelas of the face and fauces, which terminated in death. Both kidneys presented evidences of considerable congestion; in the right one, firmly imbedded in its substance, there was a calculus the size of a lozenge, elongated, curved at its extremity, and the whole cortical substance of the organ appeared to be undergoing the process of granular degeneration. The mucous membrane lining the pelvis of the kidney and commencement of the ureter was of a dull white colour, and slightly thickened.

These cases present many points of interest, some common

to both, others peculiar to each. The appearance of both these patients was strikingly characteristic of what Dr. Prout has termed the phosphatic diathesis; they were both unhealthy-looking, as if labouring under some form of cachexia. The temper of the last patient, Clarke, was peculiarly fretful, irritable, and morose; the nurse, and even the patients, complained that nothing would please him; he was always grumbling. The most constant complaint in both cases was of pain in the back, and this is a symptom well worth investigating; as Sir Benjamin Brodie truly observes, "there is nothing in practice which you will be so often consulted about as pain in the loins." The gouty debauchee, the married female, the hysterical girl, the athletic man, the victim of lumbago, all complain loudly of pain in the back, although there may not be the slightest organic change in the kidneys. Again, pain in the back is a prominent symptom in aneurism of the abdominal aorta, also in rheumatic inflammation of the intervertebral cartilages and vertebræ; and such is the difficulty, in some cases, of forming a correct diagnosis, that Sir B. Brodie with great candour mentions a case which he and Dr. Prout diagnosed and treated as calculus, which ultimately proved to be caries of the vertebræ; so that we should be slow in concluding that there is disease of the kidney because there is constant pain of the back, unless we have other symptoms to guide us, as in these cases, where we had a very important additional symptom, viz., nausea and vomiting, particularly well marked in Clarke's case, and which was caused, I feel confident, by the mechanical irritation of the tubular structure of the kidney by the phosphatic crystals; and in this irritation the stomach participated, through the influence of the splanchnic nerves, from which both the renal and gastric plexuses are derived. This symptom is also of great use in aiding us to distinguish the symptoms and signs caused by the presence of a calculus from the effects of a simple chronic nephritis. Here also there is languor, a cachectic appearance, pain in the loins; the

urine pale-coloured, alkaline, phosphatic, occasionally albuminous, but without vomiting, so that the mere secretion of alkaline urine is not sufficient to cause vomiting. It is of great importance in all these cases to ascertain whether the urine is secreted alkaline, or whether it has become so in consequence of chemical changes taking place in it after its secretion. This distinction has been particularly insisted on by Rayer, who considers that urine which is secreted alkaline is an important indication of simple chronic nephritis, and particularly so as there is often no other very obvious sign of the disease. The inflammation runs a very insidious course; they complain of some languor, look out of health, may or may not have some aching about the loins. The bronchitis which Clarke complained of on admission I regard also as another of those sympathetic irritations, intimately connected with the morbid state of the nervous system; for there was no proportion between the apparent suffering caused by the bronchitis and the physical signs on auscultation, the former being excessive, the latter trivial; nor did the symptoms yield to the usual treatment with the facility we would expect in a case of simple bronchitis.

The physical and chemical characters of the urine were highly important as in both cases, it was very pale in colour, alkaline, albuminous, and loaded with phosphates, affording a good example of the co-existence of those deranged actions by the influence of which the phosphates are deposited and albumen appears as a morbid product in the urinary secretion, and exhibits one of the numerous complications which may accompany albuminous action. This coincidence involved some nicety in the examination of the urine; for as the fluid was alkaline, it was not coagulated by heat, which merely rendered it turbid by causing a deposition of the phosphates, so that I had first to dissolve them in acetic acid, which has no effect on albumen, and then I coagulated the albumen with dilute nitric acid, thus proving the existence of both abnormal elements in the urine. Another important feature in the urine, was the

nature of the deposit; in the first case it consisted of a dense, ropy mucus, as was proved by Mr. Moore, to whose valuable assistance in the chemical and microscopic examination of urine, on this as well as on other occasions, I feel much indebted, who found the addition of acetic acid to corrugate it, and form a peculiar brainlike substance. This ropy mucus always came out with the first gush of urine. Now these points are of importance to bear in mind, as it is laid down in many books as a means of diagnosis between disease of the kidney and that of the bladder, "that in chronic inflammation of the pelvis, and calices of the kidneys, there is dysuria, with a deposit of true pus which flows *after* the urine; while, in catarrh of the bladder, the deposit is glairy and viscous, consisting of ropy mucus," but in this case the deposit was ascertained (by Mr. Moore) to be mucus, not pus, and the chief part of it came out before the gush of urine, as I had frequently asked the patient about it, and he told me it always preceded it: but we should bear in mind, that a glairy appearance may be caused in renal pus by the presence of a certain amount of alkali in the urine; and I believe, as a general rule, whether pus or mucus comes from the kidneys, ureter, or bladder, it is always passed mixed with it, though in greater quantity, towards the end of the emission. The urine in the second case, that of Clarke, was also peculiar in depositing a copious sediment, though of a very different nature, as in his urine there was no mucus, but a copious deposit of earthy and alkaline phosphates, forming a thick stratum like white gravel at the bottom of the vessel. Now this is a rare circumstance when the urine is pale, abundant, and of low specific gravity, for though this condition of the urine is generally considered to be characteristic of what is termed the phosphatic diathesis, and to consist in an excess of phosphates, yet it really is not the case. This has been satisfactorily proved by the experiments of Dr. Aldridge, detailed in the "Dublin Journal of Medical Science" for March, 1843. He states "that in place of being increased, the quantity of phosphates in the urine is

actually generally diminished in the so-called phosphatic diathesis;" and Rayer, in his valuable work on diseases of the kidney, dwells particularly on this state of the urine, as strongly indicative of a chronic inflammation, or a subacute nephritis; in fact, the term "phosphatic diathesis" does not necessarily imply any actual excess of phosphates in the urine, while it recognises their appearance as a deposit, determined by a deficiency of acid, or by alkalescence of that fluid. Earthy phosphates are very insoluble in alkaline fluids, and very soluble in dilute acids, so that the slightest trace will be made visible if the urine becomes alkaline, and a great excess will be concealed if the urine is normally acid; so that, as has been well proved by Dr. Bence Jones, their non-appearance does not argue their absence, nor their appearance their presence in excess. In fact, though variations in the acidity and alkalescence of the urine are common, yet an excess of the phosphates is very rare. It may appear strange that some of the symptoms generally considered pathognomonic of calculi in the kidneys were absent, such as pain shooting down the course of the ureters, retraction of the testes, bloody urine; but I presume that the calculus not having entered the ureter sufficiently accounts for the absence of pain in this direction, and the nature of the calculi being phosphatic and free from asperities accounts for the absence of hæmaturia. It is indeed surprising to what a size phosphatic calculi may form in the kidney without the occurrence of hæmorrhage, or even any serious symptomatic irritation, as long as they remain confined to the gland. If they are too large to pass into the ureter, they generally cause some disturbance, but not at all proportionate to their size, as, their increase being generally very slow, the organs gradually become accustomed to the stimulus, and accommodate themselves to the pressure, till the organization and consequently the functions of the kidney are destroyed; but if the calculus be dislodged or fractured, as in the case of a fall, or strain of the lumbar muscles, they are attacked with severe

pain in the back; vomiting, often inflammation of the kidneys, and suppression of urine, followed by coma and death, or suppuration of the kidney may take place; or they may escape with merely an attack of violent pain, as occurred in a case which Dr. Edwards Crisp has given me the particulars of. A gentleman, sixty years of age, when jumping over a flower bed in his garden, was seized with a sudden and violent pain in his right loin, which continued for several hours, but gradually subsided. He died about twelve months after, of disease of his heart, and in the pelvis of the right kidney a large calculus was found, which was fractured in the middle. The malformation of the kidneys in the first case, with their anomalous position lying across the vertebræ, ought not to be considered as a mere anatomical curiosity, for it might occasion a serious error in diagnosis, either by causing the displaced kidney to be mistaken for some morbid tumour in the abdomen, or might divert our attention from the true source of the disease, in cases where the symptoms should lead us to suspect a disease of the kidneys; thus Rayer, in his valuable work(*a*), gives the partilars of a case of inflamed horse-shoe kidney, which lay across the spine. The patient complained of severe pain in the umbilical region, but none in the lumbar; the disease of the kidney was not suspected, though the urine was purulent. In my case the malformation proved of assistance in making the diagnosis, as, from the kidneys being brought forward in front of the spine, I was thus enabled to detect the calculus. *I am not aware of any other case in which a calculus has been detected by feeling it through the abdominal parietes anteriorly.*

As for the treatment in these cases, acids appear to be chiefly indicated,—rather as general tonics in improving digestion, than with the hope of rendering the urine acid,—together with good animal diet, cider, and Rhenish wines. I generally prefer the dilute nitric acid, as it agrees well with the stomach. When the secretion

(*a*) *Maladies des Reins*, vol. i. p. 406.

of ropy mucus, or muco-purulent deposit, is very great, I have found benefit from the balsam of Tolu, alternately with preparations of Buchu. Pareira brava, bark in effervescence, turpentine, cubebs, copaiba, are recommended, but great caution should be observed in their use, lest they induce severe inflammatory action, for though the secretion of mucus or muco-pus may be diminished by their use, yet the paroxysms of pain are often increased. Opium, camphor, and hyoscyamus, are valuable auxiliaries, with mild counter-irritation, and belladonna to the loins. Our treatment should be palliative and preventive; and I cannot conclude this paper better than by referring to the latest opinion(*a*) of Rayer on this subject, though I think he overrates the frequency of inflammation of the kidneys as the cause of alkaline urine, and undervalues the use of acids.

Unless the examiner is well versed in the microscopic investigation of urine, he will sometimes find it difficult to form a correct diagnosis, particularly in the case of phosphates, for when they occur as minute acicular prisms, they are often crystallized like uric acid on one of the fine transparent hairs which are found in urine, and the irritation and excitement of a calculus often causes the urine to be loaded with lithate of ammonia; but the history of the case, the absence of bloody urine, and the invariably colourless crystals of the phosphatic magnesian salt, will be sufficient to prevent his making an error.

The illustration represents the kidneys of the full size, with the aorta in the centre, giving off the renal arteries. A section is made of the transverse portion, showing the substance of the kidney continued across the spine, sacculated and filled with calculi, while the ureters are represented in their natural situation, contrary to what usually occurs. The calculus, figured the natural size, was impacted in the pelvis of the right kidney, and had three large branches projecting into the calices.

(*a*) See Ranking's Retrospect, vol. ix. p. 73.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Physiological, Anatomical, and Pathological Researches. By John Reid, M. D., Chandos Professor of Anatomy and Medicine in the University of St. Andrews, &c. &c. Edinburgh. 8vo. pp. 659.

Hand-Book of Physiology. By William Senhouse Kirkes, M. D., assisted by James Paget, Lecturer on General Anatomy and Physiology at St. Bartholomew's Hospital. London. 12mo. pp. 705.

In a simply critical capacity we have but little to do with Dr. Reid's work, inasmuch as its major portion is already well known to every one conversant with the elements of physiology. The researches of this most distinguished experimentalist are part and parcel of the science he has illustrated. Criticism has long stamped them with her highest approbation, and placed them in the very foremost rank of British contributions to anatomico-physiological science. In collecting, then, his various papers, and publishing them in a separate form, the late Dr. Reid has not only done justice to his own well-won fame, but rendered an inestimable benefit to the numerous students of his works, among whom we must rank ourselves. Until the appearance of this volume, it may be safely asserted, that none but professed physiologists were aware of the number and importance of Dr. Reid's contributions to medical science, while those who were acquainted with their existence found such difficulty in consulting them, from their being scattered through so many journals, that it is probable no modern medical writer was so often spoken of and quoted, and at the same time so little read, as the author of these researches.

This volume is an *actual* republication. With mere verbal alterations, the papers are printed just as when they were

originally brought before the world. We expected no less from a man of Dr. Reid's honesty, though the neglect of this has been sufficiently common in medical literature, and has been the fruitful source of various and lengthened disputes, injurious alike to the profession and to science, about the paternity of certain discoveries. But in thus presenting to his readers the account of his researches, as originally promulgated by him, Dr. Reid has not neglected the more recent contributions to his science, a slight *resumè* of which, and of their relations to his own investigations, has been appended to the various papers. These, though not full, will be found of great use to the student who for the first time peruses Dr. Reid's Essays. We can imagine the proud satisfaction of our author in the reflection, that almost every investigation, since the period of his own researches, has not alone confirmed their accuracy, but derived support from their steady light.

The first Essay, on the "Relation between Muscular Contractility and the Nervous System," contains, perhaps, the most perfect demonstration that has ever been offered of the truth of the Hallerian doctrine of the "vis insita." Probably no point in physiology has been more contested in late days than the problem whether contractility is a property of muscular fibre, or of the nerves which supply the muscle. Very recently(*a*) the whole subject of the relation of spinal paralysis to muscular contractions has been brought before the profession by Drs. Todd and Marshall Hall, and it is impossible to doubt that the result of their somewhat warm discussion has been decidedly to confirm the experiments and inferences of Dr. Reid, as given in the Essay(*b*) before us. In reference to the experiments and reasonings of those who have contended that the *nerves* supply the contractile power to muscles, and that section or ligature of a nerve produces paralysis in the muscles to which it is distributed, Dr. Reid makes the following remark, which, indeed, may be termed the *proposition* of the Essay:

"To those arguments it may be replied, in general terms, that all that can be meant by the term paralysis, in the cases referred to, is, that the muscles have ceased to act in obedience to the mental act of volition; for, if mechanical excitation be applied to the nerve, below the point where it has been cut across, or included in the ligature, within a certain period of time after the muscles have been thus rendered quiescent, equally active and durable contractions can be produced in the muscles said to be paralysed, as in other muscles

(*a*) Medico-Chirurgical Transactions, vols. xxx. and xxxi.
(*b*) The date of the original essay is 1840.

whose nervous connexion with the central organs of the nervous system has been left untouched; and that the subsequent disappearance of the contractility in muscles thus insulated from the central organs of the nervous system is due to the imperfect nutrition that follows a state of inaction, in whatever organ of the body this may be induced."—p. 4.

Of the first part of this proposition Dr. Reid gives sufficiently ample proof. He removed a portion of the large nerve of one limb of a frog, and simply exposed the corresponding nerve of the opposite limb. The muscles of both were then galvanized until they had ceased to contract, and it was ascertained that the contractility returned as quickly and as strongly in the one limb as in the other, though in one of these the muscles had been insulated from all connexion with the nervous centre; the property of contractility, therefore, argues the author, cannot be derived from the central organs of the nervous system. Dr. M. Hall has objected to these experiments, that the division of the nerve may have acted *as a shock* upon the muscles to which it is distributed, and that the re-appearance of the muscular contraction after a few days may be due to the passing off of the effects of the shock,—a view which is altogether disproved by Dr. Reid's subsequent experiments, in which he exhausted the contractility four different times, and yet each time it returned, though the trunk of the nerve had not been disturbed after the first part of the experiment, when a portion of it was removed. In reference to the latter part of the proposition, Dr. Reid alludes to Müller's well-known experiments, and forcibly objects that this author has not taken into consideration the remarkable effects of inaction upon the structure of muscular fibre, a circumstance which, indeed, altogether vitiates the conclusions of that physiologist. Microscopic examination has proved decisively that long inaction, even when the communication of the nervous centres with the muscle is entire, produces the most remarkable alteration in the minute structure of muscular fibre. Dr. Reid removed a portion of the sciatic nerve in a rabbit; seven weeks afterwards the animal was killed, and the muscles of both posterior extremities were exposed, and irritated by the direct application of the wires of a galvanic battery to the muscles. The muscles of the paralysed leg contracted very feebly, while those of the other leg were thrown into powerful contraction. "The muscles of the paralysed member were evidently much smaller, paler, and softer than the corresponding muscles of the opposite leg." The muscles of the two legs were weighed respectively; those of the sound leg weighed 327 grains, while those

of the paralysed limb weighed only 170 grains. We are further told that, under the microscope, "the muscular fibres of the paralysed leg appeared considerably smaller, had a somewhat shrivelled appearance, and the longitudinal and transverse striæ were much less distinct than in the muscles of the sound leg."—p. 10. To decide whether or not this imperfect nutrition depended upon any supposed nervous influence flowing along the nerves to the muscles, and not merely upon inaction, the spinal nerves in four frogs, as they lie in the lower part of the spinal canal, were cut across, and both posterior extremities were thus insulated from their nervous connexions with the spinal cord. The muscles of one of the paralysed limbs were exercised daily by a weak galvanic battery, while the muscles of the other limb were allowed to remain quiescent; at the end of two months the muscles of the exercised limb retained their original size and firmness, and contracted vigorously, while those of the other limb had shrunk to one-half of their former bulk. In a note appended to this essay Dr. Reid mentions some of the more recent investigations into this subject; all of which go to prove the correctness of his own views. We may here refer especially to the observation of Mr. Bowman, who has seen contractions in separate fasciculi of muscles under the microscope, after their complete isolation from the nervous and vascular tissues. Our author remarks in conclusion upon the important application of some of these experiments to the treatment of disease. Not the least interesting feature in Dr. Reid's writings is the constant reference to practice; in his hands truly physiology is made what she ever ought to be, the handmaid of the medical art. Practical men are now quite agreed upon the importance of keeping up the proper nutrition of the muscles, while the motive power is in abeyance, knowing well by experience, that the restoration of this latter is not necessarily followed by the disappearance of the paralysis, except this condition has been maintained, either by the efforts of art, or, as in some cases, by the morbid processes themselves.

The second essay is of still greater importance, and is, perhaps, one of the ablest of Dr. Reid's contributions to physiological science. It is "On the Order of Succession in which the vital Actions are arrested in Asphyxia." In spite of the numerous investigations into this point since the time of Bichat, it must be allowed that a great deal yet remained to be done, and that what we did know was scarcely established upon a firm basis. It would be difficult to imagine a man more fitted to guide us in this most complex investigation than the author of these

researches. Independently altogether of his abstract merits as an experimentalist and medical reasoner, the minute attention which he has given to the anatomy and physiology of the principal organs concerned(*a*) gives us the greatest confidence in his observations upon the subject of asphyxia.

The essay is principally directed to the solution of the two following points: the nature of the impediment to the circulation of the blood through the lungs, and the cause of the arrestment of the sensorial functions. The impediment to the free passage of the blood through the vessels of the lungs, and its consequent stagnation in the right side of the heart, have been attributed to three causes, "the cessation of the mechanical movements of the chest, the effects of the venous blood upon the contractility of the heart, and the difficulty of transmitting the venous blood through the capillaries of the lungs, when the chemical changes which go on there between the blood and the atmospheric air have ceased." Haller held the opinion that the stoppage of the mechanical movements of the chest was the cause of the phenomena stated; a view which is now altogether abandoned. Goodwyn's argument, that the circulation through the lungs continues when an amount of fluid is contained in the chest sufficient to compress the lungs to the extent which occurs in expiration, is almost decisive against Haller's doctrine. But Professor Alison's experiment leaves no room for doubt. He confined a rabbit in nitrogen gas, until its respiratory movements became laboured and insensibility was approaching. The animal was then withdrawn from the glass jar, the brain was crushed by a blow with a hammer, and the chest was immediately laid open. The quantity of blood in the right side of the heart preponderated considerably over that in the left. Here the respiratory movements had not been interrupted until the animal had been deprived of life, and the circulation in a great measure stopped, and yet we find the same phenomena which have been attributed to their suspension. We can hardly avoid Dr. Reid's conclusion, "that the accumulation of blood in and around the right side of the heart is dependent upon the cessation of the chemical changes between the blood and atmospheric air in the lungs, and not upon the arrestment of the mechanical movements of the chest."

But this is still further proved by the admirable experiments of Dr. Reid himself, who adopted the following procedure. A tube with a stop-cock was fixed into an opening in the trachea of a

(*a*) See the articles "Heat" and "Respiration" in the Cyclopædia of Anatomy and Physiology.

rabbit, and one of Poiseuille's hæmadynamometers were introduced into the femoral artery, for the purpose, we are told, of obtaining definite information upon the force with which the blood was transmitted along the arterial system. The stop-cock was then shut, and when the respiratory process had been suspended sufficiently long to cause a decided fall in the column of mercury supported by the blood of the femoral artery, a bladder full of pure nitrogen was fixed in the tube of the trachea. The mercury continued to fall in the instrument, descending two inches in one minute after the adjustment of the nitrogen bladder to the trachea, as we learn by the table with which Dr. Reid has furnished us. A bladder of the same size, but filled with atmospheric air, was now put into connexion with the trachea, the mercury rose very rapidly *immediately* after the atmospheric air had entered the lungs, and acted upon the blood, and stood five inches higher than when the animal was breathing nitrogen. This experiment was repeatedly performed with the same results. Here, too, the respiratory movements continued during the breathing of the nitrogen gas, but they failed to renew the free circulation of the blood, while the breathing of atmospheric air was instantly followed by an increase of pressure upon the arterial coats. Dr. Reid's observation upon the *rapidity* of the rise of the mercury after the animal was allowed to breathe atmospheric air is of great importance.

Since the date of this paper, Mr. Erichsen has published some inquiries upon the phenomena of asphyxia, in which he has advocated the theory that the arrestment of the circulation through the lungs is owing to the circumstance that the venous blood acts as an excitant upon the contractility of the ultimate ramifications of the pulmonary veins, and this causes an obstruction to its progress. But Dr. Reid objects that the *simple contractility* of arteries manifests itself by slow contractions, followed by equally slow relaxations: "If then the minute radicles of the pulmonary veins be stimulated to contraction by the venous blood, relaxation ought not to follow instantly upon the withdrawal of this excitant; and the entrance of pure atmospheric air into the lungs ought not to be instantly followed by the free passage of the blood from the right to the left side of the heart," which we have seen to be the case. Reference is also made by the author to an experiment of Bichat, one which he has frequently repeated, in which the trachea was obstructed, until a current of dark blood feebly circulated in the arteries, when a large artery was opened, and instantly, upon the admission of atmospheric

air, blood of a bright red colour sprang from the divided artery, and with greatly increased force.

It was in the experiments first alluded to that Dr. Reid made the unexpected observation which is now incorporated as an important fact with general physiology, that when the blood had become dark in the arteries, and the animal functions had been suspended, the mercury actually stood higher in the instrument, and the large arteries, became more tense for about two minutes after the animal had become insensible than before the stop-cock in the trachea was shut, and while the animal was breathing atmospheric air freely. This, which was so little to be anticipated from *à priori* reasoning, Dr. Reid observed so uniformly in his numerous experiments, that it became necessary to set about an explanation of the phenomenon. There seemed but one mode of accounting for this appearance, namely, that the same impediment which existed to the circulation in the pulmonic capillaries existed also in the systemic capillaries, and that, therefore, the force of the left ventricle was principally concentrated on the arterial system. This surmise was proved to be correct by placing a hæmadynamometer in the vein of the opposite limb, when the remarkable difference between the height of the mercury in the two instruments gave the clearest proof of the existence of obstruction in the systemic capillaries. So far from being *exceptional*, therefore, this phenomenon offers additional proof of the accuracy of Dr. Reid's main doctrine, that the interruption of the due relation, *quoad* nutrition, between the solids and fluids, lies at the very basis of the pathology of asphyxia. We must refer also to another striking observation made during these experiments. When the animal made an attempt at expiration, and commenced to struggle, the mercury rose from half an inch to nine inches, and in one experiment to ten inches; while, on the other hand, during each attempt at inspiration, and during quiescence, the mercury descended to the same extent, "making a most material disproportion in the extent of the pressure upon the inner surface of the arterial system of vessels."

The opinion that the heart's action ceases because it cannot be excited to contract by venous blood is now known to be erroneous. Dr. Reid's own experiments are conclusive as to this point. Bichat found that in an asphyxiated animal dark blood at first traverses the lungs to reach the left side of the heart, and may be projected, for a short time, with considerable force, from a cut artery; and that, even when the heart has ceased to beat, it may be re-excited to contract by the injection of venous blood along one of the pulmonary veins

towards the left side of the heart. It is scarcely necessary to remind our readers that Bichat's opinion as to the cause of the accumulation of the blood in the right side of the heart was, that it was owing to the circulation of dark blood in the bronchial arteries, the cessation of the excitation of the lungs by atmospheric air, and the enfeeblement of the heart by the circulation of venous blood in its muscular tissue; aided by the circumstance that the systemic ventricle more easily overcomes the resistance offered by the systemic capillaries than the veins and pulmonic ventricle can overcome that arising from the capillaries of the lungs. We consider this latter a much more important element in the production of the phenomena of asphyxia than is generally imagined. We find that it is only after an interval of several minutes that there is evidence of increased arterial pressure, from the cessation of the vital actions ever going on in the healthy condition, between the capillaries and the tissues; the same state, it must be remembered, which exists in the lungs from the absence of oxygen. But it is quite certain that this condition has *begun to arise* considerably before this period. We think an experiment devised by Dr. Reid for a very different purpose gives considerable strength to this surmise. A stop-cock was put in the trachea as before, and the femoral artery was then laid bare. The pulse now ranged from 105 to 120 in a minute, and the respirations were very short and rapid. At the end of the first half minute after the stop-cock was turned, the pulse was 92. At one minute and a half the pulse was about 120, the animal had commenced to struggle, and the blood in the artery was decidedly dark. At the second minute the blood was nearly as dark as in the accompanying vein, but the struggles of the animal rendered it impossible to count the pulse. At the end of two minutes and a half the animal had ceased to struggle, was evidently insensible, and the pulse was 42. In a second experiment the pulse rose thirty-four beats at the end of one minute after the stop-cock was closed, and when *the blood had become decidedly darker*. On referring to Table I. we find that in *one* minute after the stop-cock was turned, "the artery becoming a little dark," the hæmadynamometer ranged between 3·0 and 7·0 inches, it having stood at 5·5 the instant the stop-cock was turned. At the termination of another half-minute it ranged from 3·0 to 9·0; at the end of two minutes it stood at 12·0. We have seen that the general result of the experiments on the pulse first named was, that it is at its highest range at about one minute from the commencement of the experiment. The left ventricle was obviously struggling to its utmost to

overcome the obstruction commencing to exist in the systemic capillaries, and was successful ; but as the process of asphyxia went on it ceased to be competent to overcome the accumulating difficulties of its position ; the pulsation diminished in frequency, and the hæmadynamometer showed the extraordinary arterial pressure we have alluded to.

These experiments offer a very exact similarity to certain morbid processes. In some cases of pulmonary emphysema with intense bronchitis, the pulse is quick at first, and during this period the cerebral functions are well performed; but towards the termination of the disease, the pulse often becomes slow, and concurrent with this are first observed stupor, delirium, and sometimes, as we have recently seen, convulsions. There can be no doubt, whatever may be the opinion held upon this particular point, that Bichat had clearer ideas upon the nature of asphyxia than any author who preceded him, and than many who have followed him. That he had not distinct notions concerning the minute changes in the capillary system is true, but this is a knowledge which has resulted from very recent investigations. To Graves, Alison, and Draper, among many others, we owe our enlightenment on this abstruse subject. Indeed we have always considered the clear views we now have of the functions of nutrition and secretion, as compared with those of our ancestors, among the best and most stable consequences of modern physiological research.

The cause of the arrestment of the sensorial functions in asphyxia has given rise to almost equal discussion. Bichat held that the circulation of venous blood in the arteries of the brain was the cause of this phenomena. Dr. Kay believed they were owing principally to a diminished supply of blood to the cerebrum. Dr. Reid argues in favour of Bichat's doctrine, but asserts that the effects of the circulation of venous blood in the brain are not owing to any noxious influence of that blood upon the nutrition of the organ, but to its not possessing the power of *exciting the brain to action*, a matter of which Bichat spoke doubtfully. Our author has very properly separated the *sensorial* functions from those of the medulla oblongata in these experiments,—the former, being suspended before the latter, as evinced by the respiratory efforts, are modified to a very great degree ; and he expresses himself as satisfied "that the function of respiration is much enfeebled at a period of the process of asphyxia when this cannot be explained by any diminution in the quantity of blood sent to the medulla oblongata." He adduces in support of his own views, and against Dr. Kay's doctrine,

the experiments on the pulse in different stages of asphyxia to which we have already referred.

We must confess that our leaning is towards a middle view between those of Bichat and Kay. Dr. Reid himself does not appear to us to have come to an absolute conclusion on this point, for while he holds that the arrestment of the sensorial functions does not depend upon any diminished transmission of blood through the vessels of the brain, he admits that the pulse has become less frequent about the time that the insensibility has supervened. Subsequently he states "that it is quite possible that the ultimate cessation of the functions of the medulla oblongata *may be hastened* by the diminished quantity of blood sent along the arteries supplying it."—p. 36. We hardly think Dr. Reid is fully aware of the remarkable effect of slowness of the pulse upon the cerebral functions. Apoplectic attacks have been recorded(*a*) as of frequent occurrence in persons in whom this peculiarity existed. In poisoning by digitalis, while the pulse is perhaps beating at 40 in the minute, we observe giddiness, stupor, convulsions, coma, and death. In narcotic poisoning, in general, we believe slowness of the pulse to be the immediate precursor of death(*b*). Our conviction is, that in these cases, as well as in the experiments before us, the functions of the encephalon are partially suspended by the circulation of venous blood, while their complete cessation is produced by a diminished supply. It is highly probable that the former stands to the latter as cause to effect; that the cerebral functions, impaired by venous blood at first, so re-act upon the heart as to produce at least diminution of supply, and therefore entire suspension; and this is, in spite of his objections to Dr. Kay's experiments, pretty nearly Dr. Reid's own opinion. We shall now quote his *resumè* of this subject, with which we shall conclude our remarks upon this Essay, recommending it strongly to the perusal of our readers:

"Dark blood is at first transmitted freely through the lungs, and reaches the left side of the heart, by which it is driven through all the textures of the body. As the blood becomes more venous, its circulation through the vessels of the brain deranges the sensorial functions, and rapidly suspends them, so that the individual becomes unconscious of all external impressions. The functions of the medulla oblongata are enfeebled about the same period that

(*a*) See Dublin Hospital Reports.

(*b*) We were lately struck by the fact in a case of poisoning by cyanide of potassium.

the sensorial functions are arrested, but are not fairly suspended for some time longer. Immediately after the sensorial functions are suspended, and the blood has become still more venous, it is transmitted with difficulty through the capillaries of the lungs, and consequently begins to collect in the right side of the heart. A smaller quantity of blood must now necessarily reach the left side of the heart; and this diminution of the quantity of blood sent along the arteries, conjoined with its venous character, and the ultimate arrestment of the circulation, being circumstances incompatible with the manifestation of vitality in the other tissues of the body, general death is sooner or later induced."—p. 38.

The third paper in the volume before us, "On the Effect of Venesection in renewing and increasing the Heart's Action," though short, abounds in matter for reflection. The experimental results of Dr. Reid perfectly coincide with our own ideas, drawn from the observation of disease. The remarkable effect of small, well-timed (chiefly local) blood-letting, in certain conditions of embarrassed circulation complicated with pulmonary mischief, though by no means recognised by the profession at large, seems to us to support very strongly the speculative inferences of Dr. Reid. Our knowledge of the true pathology and diagnosis of these disorders has probably rather retarded our treatment of them. Those who are sceptical upon this point we should recommend to peruse the cases of authors twenty or thirty years ago, when the diagnosis of these diseases was imperfect, and when blood-letting was not uncommonly practised as a part of the treatment. We have seen comfort obtained, and even sometimes death manifestly kept at bay for a long period, by a timely cupping between the scapulæ, an infinitely safer proceeding than the long-continued use of digitalis, now so fatally common.

"Two rabbits were killed by a blow on the head sufficient to injure a portion of the brain. On exposing the heart, its movements were feeble and the right side of the heart was engaged. The escape of blood from the external jugular vein was followed by a decided increase in the number and strength of its contractions, which lasted for a considerable time."—p. 54.

The same result was observed in many other experiments. Dr. Reid observed that the effect of artificial inflation of the lungs in renewing the circulation, though sufficient if commenced when the contractions of the heart are still going on vigorously, are materially assisted, when these are less active, by disgorging the right side of the heart. He believes that bleeding from the external jugular vein, in cases of asphyxia, is principally efficacious by its power of renewing the heart's

action, and with this view recommends that the blood should be allowed to flow from the lower orifice, "and from that orifice alone," if we are anxious not to draw much blood. He gives us the proviso that "no air should be allowed to pass into the vein," but the obvious difficulty in attaining this object strikes us as being a practical objection to this procedure.

Of the next two papers, "On the Functions of the eighth Pair of Nerves," being by far the longest and most important in the work, we feel it to be unnecessary to say anything. On them will be mainly founded Dr. Reid's fame as a physiologist. When we consider the difficulty of his subject, the contending opinions and experiments of previous investigators, the vast interest attached to an exact knowledge of the functions in question, it is scarcely possible to over-estimate the debt we are under to the man who, by a most searching, patient, and profound experimental analysis, has scarcely left room for doubt upon the many subjects he has taken the pains to investigate. That these researches are eminently practical we need only refer to his experiments upon the effects of section of the vagus upon the lungs, and of the inferior laryngeal nerves upon the glottis; that we are not setting too high a value upon these researches, we must ask our readers to refer to the writings of any recent physiological author for ample proof. We think the profession is indebted to Dr. Carpenter for his early appreciation and well-expressed confidence in these experiments(*a*). Dr. Kirkes, the "Handbook of Physiology," which we have placed at the head of this article, has also drawn largely from this rich mine. As the former has long been the students' text-book, and as the latter bids fair to possess a similar place in their library, we repeat our unwillingness to bring before our readers what must be well known already to the least learned among them.

To these follow short papers "On the Effects of Lesion of the Trunk of the ganglionic System of Nerves in the Neck, upon the Eye-ball and its Appendages," and "On some Points in the Anatomy of the Medulla Oblongata." In this latter Dr. Reid has insisted upon the motor character of the olivary body, and has traced with his usual care and accuracy the attachments of the motor, encephalic, and spinal nerves to this column, and to its upward prolongation into the encephalic mass. The following essay, being No. VIII., "On the Anatomical Relations of the Blood-vessels of the Mother to those of the Fœtus in the human species," is also one of Dr. Reid's best known contributions to Science. The view he has here advo-

(*a*) Human hysiology, first edition.

cated of the relation of the fœtal villi to the vascular system of the mother is the received opinion in this country at present. His main proposition is, that the placental tufts project to a varied extent into the utero-placental veins; the inner surface of the vascular system of the mother being everywhere reflected upon the outer surface of these tufts. He speaks of this coat of the maternal vascular system forming "a large sac, which is intersected in many thousand different directions by the placental tufts." Some diagrammatic sketches are given of the minute anatomy of this part of the subject, which will be found copied in Dr. Kirkes' Handbook, who has for the most part followed our author. Both these writers have referred to Mr. Goodsir's admirable observations upon the minute structure of the fœtal villi, which we may consider the crowning illustration of the cell-theory in general, and of the ingenious germinal-centre theory of that anatomist in particular. Dr. Kirkes has given these views in a very succinct and clear manner:

"It would seem that at the villi of the placental tufts, where the fœtal and maternal portions of the placenta are brought into close relation with each other, the blood in the vessels of the mother is separated from that in the vessels of the fœtus by the intervention of the distinct sets of nucleated cells. One of these belongs to the maternal portion of the placenta, is placed between the membrane of the villus and that of the vascular system of the mother, and is probably designed to separate from the blood of the parent the materials destined for the blood of the fœtus; the other belongs to the fœtal portion of the placenta, is situated between the membranes of the villus and the loop of vessels contained within, and probably serves for the absorption of the material secreted by the other set of cells, and for its conveyance into the blood-vessels of the fœtus. Between the two sets of cells, with their investing membrane, there exists a space, into which it is probable that the materials secreted by the one set of cells of the villus are poured, in order that they may be absorbed by the other set, and thus conveyed into the fœtal vessels."—p. 643.

The next paper, "On Injections of the Vessels of the Fœtus," we shall pass over with the remark that Dr. Reid has, by his well-contrived injections, shown to a demonstration that the Eustachian valve actually possesses the power now generally ascribed to it,—but upon which some still affect to throw a doubt,—of effectually preventing the mixture of the two currents of blood from the cavæ in the right auricle of the fœtus.

Passing over a comparatively unimportant paper, "On sensational and emotional reflex Actions," we arrive at No. XI.,

"On some Points in the Anatomy and Physiology of the Heart," and No. XII., "On the Measurements of the Heart." The former of these papers is an abstract of certain portions of the author's well-known article, "Heart," in the Cyclopædia of Anatomy and Physiology. The description there given of the precise attachment of the great vessels to the tendinous rings, and of the exact figure of these rings themselves, is now the "staple" of all demonstrators of these portions of the heart. Under the section, "Mode of Action of the Valves of the Heart," Dr. Reid states his conviction, that the opinions that the mitral and tricuspid valves are passively floated up towards and obstruct the auriculo-ventricular orifices, so as to prevent the free regurgitation of the blood into the auricles, and that the use of the chordæ tendineæ is merely to limit the movements of the valves, are erroneous; and, *per contra*, maintains with Bouillaud that these cords are the proper tendons of the musculi papillares, by whose contraction the valves are approximated. For proof he refers to the very uniform position and course of the musculi papillares and chordæ tendineæ, more particularly those of the left ventricle, that the chordæ tendineæ pass from each musculus papillaris to both lips of the mitral valve, occasionally crossing each other; "and that the posterior or smaller lip, though it may be drawn inwards so as to meet the larger and more moveable, is so bound down as to be scarcely capable in most cases of being floated up on a level with the orifice; and further, that the musculi papillares contract at the same time with the other fibres of the heart." Both Haller and Dr. Reid have proved this last statement by actual experiment. Our author professes his adhesion to the opinion that the impulse of the heart against the parietes of the chest occurs at its systole, and rejects altogether the various explanations that have been given of the particular mode in which the apex is made to impinge against the thoracic wall. To Senac's idea that this was principally brought about by the attempt of the two great vessels to *straighten* themselves, and thus the apex, describing the arc of a circle greater than the other parts of the heart, comes in contact with the side, he opposes the observation of Drs. Corrigan and Shebeare, "that the direction of the curvature of the large arteries is such, that if any effect of this kind is produced, the heart would not be carried to the left side, but in the direction of the curve, which is exactly in the opposite direction."—p. 368. Dr. Reid believes that the true explanation must be drawn from the course of the muscular fibres. We have not space to insert his minute description of these fibres, which is given in full in the excellent article just re-

ferred to; but we may briefly state that the explanation is based upon the facts, that a large number of fibres pass from the posterior portion of the septum in a spiral manner towards the apices of the ventricles, and are finally inserted, some into the tendinous rings, while others form part of the columnæ carneæ. In this arrangement the tendinous rings are obviously the fixed points, and as some of the fibres are only connected to the tendinous rings through the lax chordæ tendineæ and valves, it must follow that the force with which the contraction takes place towards the septal extremity must preponderate over the other; this being the case, "the apex must be carried forwards at the same time that it is drawn upwards towards the base."

The next brief paper, "On the Measurements of the Heart," well merits attention from those who are in the habit of recording cases. We have long been convinced of the necessity of giving something like precision to reports of certain altered conditions of the cardiac orifices, by proper measurements. At present it is impossible to rely upon the observations of any but professed pathologists of acknowledged experience, who, by habit, have obtained a certain standard of what is and what is not abnormal. But we need scarcely observe that the vast bulk of recorded cases are given by men who make, probably, but two or three *post mortem* examinations in the year. There cannot be a shadow of doubt that men fancy orifices dilated or contracted, just in proportion as the one or other view favours the explanation of the symptoms during the life of the patient; or as one or the other view vindicates the diagnosis of the observer, who is acting quite honestly, perhaps, but so wedded to the rules of dogmatic diagnosis, under which, unfortunately, this branch of medicine is suffering very heavily, that he imagines he sees all he ought to see. Careful measurement would remedy all this, and would, we firmly believe, effect considerable changes in heart-pathology as at present taught. Dr. Reid's mode, and which we can recommend to our readers from our own experience, is to cut through the auriculo-ventricular and arterial orifices, and measure the distances between the cut ends of the tendinous rings which surround these orifices.

Many interesting papers follow upon which we cannot dwell, but we feel called upon to draw the attention of the reader to No. XIX., being a "Report on the Epidemic Fever of Edinburgh." The account has been constructed from cases which occurred from the end of October, 1838, to the middle of June, 1839. The analysis of the symptoms in nearly 200 cases is given by Professor Henderson, whose departure from the walk of regular medicine we cannot help deeply deploring.

Of this analysis, excellent though it be, we have scarcely anything to say. The reports of the distinguished physicians in this country, who have successively recorded their experience of this deeply interesting disease, as is shown by the Report on the late epidemic, concluded in our present Number, leave nothing to desire on this point. We look upon the reports alluded to as perfect models of what mere observation can effect. But we presume no candid man has a doubt that, in spite of their vast opportunities, the Irish practitioners have not given us very extensive data towards the foundation of a knowledge of the pathology of the disease; indeed how could they, with the difficulties thrown in their way for making *post mortem* examinations? Here Dr. Reid comes to our assistance, and in the conclusion of this Report, and more particularly in the one following, "On the Pathology of the Edinburgh continued Fever," gives us a careful and valuable summary of the morbid appearances in a considerable number of cases. Of course we cannot pretend to give a detail of the numerous observations here brought forward, but there are two series of morbid appearances described, upon which we must make some brief remarks. The first of these consists of the description of the morbid state of the brain and its membranes. We are told that the brain was examined in forty-three out of the forty-seven cases which form the basis of Dr. Reid's summary. Of these forty-three cases increased effusion of serum within the cranium was observed in twenty-five. The most common seat of this effusion was under the arachnoid. In five out of the twenty-five the quantity of serum effused was sufficient to elevate the arachnoid above the surface of the convolutions only at the most depending parts. In other five the effusion of serum under the arachnoid was slight, but it existed in other portions of the brain. In fourteen cases the quantity of serum effused was sufficient to elevate the arachnoid above the convolutions, not only at the posterior or depending parts of the surface of the brain, but also over the middle parts. Out of the fourteen cases last referred to, the lateral ventricles contained an increased effusion of serum in five; in one case amounting to ten drachms. Now in the five cases first mentioned, the following were the symptoms during life. One is stated to have been *confused;* another was restless, and had a convulsive fit; another had stupor, became comatose, and continued so till the time of his death; a fourth had headach on admission, with muscular tremors. In the next five cases we find one is stated to have been restless and delirious the third evening preceding his death, and on the morning of that event

to have had subsultus tendinum and muscular twitches; in another, in whom the lateral ventricles were slightly, though evidently distended, without any increased effusion under the arachnoid, the patient is said to have been violently delirious, requiring coercion, the night previous to his death. In another of the five cases headach was complained of, and the pupils were contracted two days before death. Among the next fourteen cases we see recorded in almost all some well-marked "head symptoms," as stupor, coma, tremors, subsultus tendinum, contracted pupils, restlessness, &c. But in "the other cases in which increased effusion of serum was found within the cranium, they appear to have had only the confusion of thought so commonly observed in fever; and it is of importance to remark, that in one of those cases, in which one ounce of serum was effused into the lateral ventricles, and the arachnoid elevated above the surface of the convolutions at the posterior and middle parts of the brain, the disease ran its course without any marked local symptom."—p. 458.

To discover the relation which these symptoms bear to the morbid appearances we must pursue the inquiry as Dr. Reid has done, namely, investigate the symptoms in the eighteen cases in which no increased effusion of serum was found within the cranium. The first case, which occurred in a woman, exhibited "furious delirium" on the second night previous to death. In a second the patient was unable to give any account of himself on admission; the eyes were suffused, the pupils were contracted, and he had slight subsultus; he remained insensible, and died on the third day after admission. In another case the patient the evening before death had a convulsive fit, with movements of arms and lower extremities, and distortion of features, which lasted for about ten minutes; she had two similar attacks within four hours: she became insensible after the first convulsive attack, and died next morning. In the last-mentioned case, being the fourteenth in which cerebral symptoms were well developed, the patient was delirious, and attempted to escape out of a window two nights before death. It is impossible to gainsay the inference of Dr. Reid from these cases, that they afford no distinct evidence that the serous effusion was in all cases, if in any, the cause of death. This last consideration is again dwelt upon in the Report following, and the relation of the effusion of serum upon the brain to the morbid conditions of the lungs is carefully analysed; all which, though deeply interesting, we can do no more than commend to the notice of the inquirer.

The second series contains the result of the examination of

the abdominal organs. These were examined in forty-one cases out of forty-seven. The mesenteric glands were distinctly enlarged in two cases only. In two cases only were the elliptical patches of Peyer very distinctly elevated, and presented any appearance of ulceration. One of these cases had left the county of Sligo only ten days before. Of the residence of the second nothing is stated. It is remarkable that, while lesion of the intestinal canal was so rare in Edinburgh, it *prevailed* in the fever raging, at the same period, at Anstruther in Fifeshire, an account of which is given, drawn from information furnished by that accomplished anatomist, Mr. John Goodsir. In all the *post mortem* examinations obtained, which amounted to ten, Peyer's patches and the solitary glands at the lower part of the ileum were elevated and ulcerated. In the next Report Dr. Reid states, that in six only of the ninety-one cases which form the basis of the Report were the elliptical patches elevated and ulcerated, and in five of these the solitary glands were in the same condition. These six cases are divided into two classes,—into those in which the ulceration begins on the inner surface and gradually extends downwards, the interior of the enlarged patches containing no lardaceous-looking deposit; and into those in which the ulceration commences by the detachment of sloughs formed in the interior of the enlarged glands, from the breaking up of a lardaceous-looking deposit. This last division corresponds with the appearances considered characteristic of the typhoid fever of Paris. Taking together the forty-one cases of the previous Report and the ninety-one of the present, we find that, in the whole 132 cases, ulceration of the elliptical patches or solitary glands was found in eight; but in five only do the appearances exactly coincide with the description given of the typhoid fever of Paris. In two of the others the patients were not resident in Edinburgh before the commencement of their illness, in fact were Irish labourers in search of work. Dr. Reid remarks:

"During the whole three years and a half that I conducted the *post mortem* examinations in the Edinburgh Infirmary, in no single case did I observe, in any individual who had been seized with fever while residing in Edinburgh, anything resembling the changes described as occurring in the lower part of the ileum in the typhoid fever of Paris."—p. 501.

Why the typhoid form of fever should for several years past never be found in Edinburgh, while it existed at Linlithgow, Anstruther, and other places in Fifeshire, is truly worthy of investigation, as suggested in the Report before us. In the

appendix it is mentioned that in the winter of 1846–7 cases of typhoid fever became frequent; and Dr. Bennett, the successor of Dr. Reid as pathologist to the Royal Infirmary, examined during this period upwards of twenty fatal cases of fever, in which the follicular lesion of the intestines was more or less marked. These remarkable observations only still further obscure our knowledge of the intimate nature of fever, but meanwhile they must be noted for future investigators.

Several interesting papers follow, among which we would particularly point out a review of Wattman's Treatise on the entrance of air into the veins, which contains a short but clear account of almost all we know on this subject. But we must now conclude our very imperfect analysis by enjoining all who really love their profession as a science, or who wish to gain information on some of the arcana of physiology, to procure and study this most admirable work—the labours of one only lately snatched from among us, but who, by this, probably the most valuable contribution to medical science the British school has for many years given to the world, has left behind him an undying name.

On the Handbook of Dr. Kirkes we shall make but few observations, firstly, because, in its very nature, it hardly bears a regular analysis, and secondly because we had lately occasion, in discussing Dr. Carpenter's Treatise, to bring before our readers some of the more prominent and interesting facts of recent physiological research. The present work of Dr. Kirkes is a most worthy preparatory book to the larger and more comprehensive treatises of Wagner and Müller, to which we may add the giant Cyclopædia of Anatomy. In compiling his Handbook Dr. Kirkes has had the valuable assistance of Mr. Paget, which circumstance not a little enhances its value. The arrangement differs from most preceding treatises; yet we cannot but state our firm opinion that Dr. Kirkes has in this matter followed the most natural course, and one which is practically found to be most convenient for the student, for whom this book is principally intended. The first two chapters are on the chemical and structural composition of the human body, which are followed by an account, in distinct chapters, of the several organic functions, including absorption, nutrition, and secretion. It is not till the fifteenth chapter that we find the account of the nervous system and of the functions of relation. The concluding chapter is on "generation and development." Notwithstanding the opinions and practice of certain authors, we think it would be hard to prove that the most natural way of teaching physiology is to commence with

the description of the highest animal functions, to thoroughly understand which requires that the reader should be habituated to physiological reasoning. We believe our author has wisely judged the contrary is the best method, which commences with an account of those functions which, as well as being the most *general*, are also the most easily analysed and made the subject of generalization. The chapters on the blood and the circulation have struck us as being particularly full and excellent. The section on "the vital properties and actions of the blood" contains an abstract of Mr. Paget's Lectures of last year on the life of the blood, which deserve great praise. The following is a very lucid account of the principal steps in the development of the human lymph, or chyle-corpuscle, into the red corpuscle:

"The white corpuscle, at first tuberculated, containing many granules, and darkly shaded, becomes smoother, paler, less granular, and more dimly shaded or nebulous; changes corresponding with those which Mr. Wharton Jones describes as from the coarsely to the finely granular stage of the *granule-cell*. In these stages the cell-wall may be easily raised from its contents by the contact and penetration of acetic acid, or by the longer action of water. In the regular progress of development, it becomes at length impossible to raise the cell-wall from its contents. Then the corpuscles acquire a pale tinge of blood colour, and this always coincides with the softening of the shadows which before made them look nebulous, and with the final vanishing of all the granules, with the exception sometimes of one which remains some time longer, like a shining particle in the corpuscle, and has probably been often mistaken for a nucleus. The blood colour now deepens, and at the same rate the corpuscles become smooth and uniform; biconcave, having previously gradually changed the nearly spherical form for a lenticular or flattened one; smaller, apparently by condensation of their substance; more liable to corrugation and to collect in clusters; and heavier, so that the smallest and fullest coloured corpuscles always lie deepest in the field."—p. 69.

In conclusion, we have only to repeat the opinion above offered, that this treatise bids fair to obtain a place in the student's library, to which honour it has an undeniable claim.

Obstetrics: the Science and the Art. By Charles D. Meigs, M. D., Professor of Midwifery and the Diseases of Women and Children in the Jefferson Medical College at Philadelphia, &c. Philadelphia, Lea and Blanchard. 1849. 8vo. pp. 685.

Amongst our transatlantic obstetric brethren, none at the present day enjoys greater eminence or a wider fame than Dr. Charles Meigs. An author of established merit, a professor of midwifery, and a practitioner of high reputation and immense experience,—we may assuredly regard his work now before us as representing the most advanced state of obstetric science in America up to the time at which he writes.

Dr. Meigs, be it observed, is no novice at authorship, is no mere candidate for fame; the latter he has obtained. Whatever he states, therefore, comes invested with an air of authority, and will, no doubt, sway the opinions and practice of many; but on this very account it behoves us to examine the contents of his book with close scrutiny, and to declare our convictions with undisguised candour. We trust to be forgiven, should we find occasion to differ on some points with so high an authority as Professor Meigs, for whom personally we entertain every sentiment of respect and admiration.

Dr. Meigs evidently appears to be a man of original mind, who thinks and reasons for himself, and is not content to follow with undeviating steps in the beaten track of his predecessors, however eminent they may have been. Indeed he himself candidly tells us that "he is not much accustomed, in his medical transactions, to be guided by what are called authorities." Every page of his work bears this independent character, not only in the matter, but most conspicuously in the style and the language. He writes in a free, unembarrassed manner, rather unlike what we are accustomed to in scientific or medical treatises; at the same time he conveys his meaning satisfactorily, and carries the reader along without producing any of that feeling of weariness or monotony so apt to be induced by a more methodical and purely didactic style. No doubt he occasionally makes choice of words that sound a little harsh and uncouth, but for this *we* cannot in justice pass censure upon him; to do that is for an *American*, not for an Irish or an English critic.

In the preliminary observations, our author defines midwifery as "the art of assisting women in labour," and a midwife or accoucheur as "one who assumes the conduct of cases

of labour;" whilst "an obstetrician is a physician who, in addition to a general knowledge of physic and surgery, adds the special information that is necessary for one having the peculiar charge of all sexual affections, whether in the department of midwifery proper, or in other complaints of the sex." —p. 17. Now although this is a purely arbitrary distinction, having no foundation in the respective etymology of the words, still it meets our cordial approbation, and we hope it will be generally recognised, as the want of such a distinction has become a source of much inconvenience in medical writings.

Dr. Meigs divides his book into four parts, of which the first two treat of the anatomy and physiology of the organs subservient to reproduction. Part III. is on "the therapeutics and surgery of midwifery;" and Part IV. is devoted to the management and diseases of the "neonatus," or young infant. As the first part is chiefly elementary, it need not detain us long. With Baudelocque and the French school generally, Dr. Meigs recognises six positions in which the head may enter the pelvis at the beginning of labour; and to the successive changes which it undergoes during its transit, and upon its expulsion, he gives the names of *flexion*, *rotation*, *extension*, and *restitution*, this last implying the turn made by the head immediately on clearing the outlet. These four movements are simply the results of rotation upon its vertical and transverse axes; but of that oblique position of the head produced mainly by slight rotation on its longitudinal axis,—in consequence of which, one (the anterior or pubic) parietal bone is always lower down than the other,—he makes no distinct mention. To the anatomical description of the generative organs, he adds many excellent practical remarks upon the diseases and abnormal conditions of these parts. The minute structure of the Graafian vesicle and of the contained ovum, together with a short account of the discovery of the latter, is clearly and satisfactorily given. His observations on the corpus luteum fill ten pages; but nearly the half of this space is devoted to the consideration of a discovery he supposes he has made, viz., that the yellow matter is of a true vitellary nature. He thinks it is "deposited outside of the inner concentric spherule or ovi-sac of the Graafian follicle" (p. 107); and that this deposition, beginning prior to the rupture of the vesicle, tends to bring about this result, and the consequent extrusion of the ovule. The position he assigns to the yellow matter is in accordance with the views of Dr. Montgomery and Dr. Patterson; but we think that the researches of Raciborski and Cazeaux throw considerable doubt on its vitellary nature, and also upon the opinion that

its secretion commences before the discharge of the ovum. Adopting the notions of some modern continental ovologists, Dr. Meigs seems to regard the corpus luteum merely as the sign of a "finished ovulation," and remarks:

"Every oviposit is followed by a corpus luteum, which is larger or smaller according to circumstances. Many women have scarce discernible ones after conception; others have very large ones. The true and false corpora lutea differ only in magnitude, not in their essential nature."—p. 115.

Now, without for one moment disputing the common origin of the menstrual corpora lutea and those succeeding to conception, we still cannot help thinking that the physiologists who deny to the latter any peculiarities which constantly serve to distinguish them from the former have come to this conclusion more from theoretic inference than from direct observation; whilst, on the other hand, a large mass of evidence can be brought forward to prove that the *true* corpus luteum (or that resulting from fecundation of the ovum) does possess many striking and unequivocal characters. Of the many dissections we have witnessed of women dying during pregnancy or the puerperal state, there was not one in whom a well-marked true corpus luteum was absent; and, further, although we have examined the ovaries of numerous females whose menstruating functions had been duly performed up to the period of dissolution, yet in no single instance was there anything to be found, which could be mistaken for a *true* corpus luteum.

The second part of the work commences with the subject *Menstruation*, which our author treats in a very perspicuous manner. Thanks to the labours of Coste, Lee, Negrier, Gendrin, Jones, Raciborski, and Pouchet, we have at last obtained clear and rational views respecting the functions of the unimpregnated uterus and ovaria; and the theory of menstruation, once so involved in doubt and obscurity, now admits of a simple explanation by reference to that natural law, the discovery of which crowned the researches of the above-named physiologists. Rigidly adhering to the doctrine they have established, Dr. Meigs describes the process of menstruation as essentially consisting in the periodical evolution and expulsion of an ovule; the menstrual fluid being an effusion or extravasation, but not a secretion, from the inner surface of the uterus, the result of a local plethora or state of periodic hyperemia of the reproductive organs. In whatever way we may regard it, this is certainly a most important discovery in physiology,

and one that has conferred immense service on practical medicine. This Dr. Meigs forcibly points out by many apt illustrations. In connexion with the subject under review, we may be permitted to remark, that Dr. John Power, of London, appears to have been the first person who advanced the doctrine of menstruation being in some way dependent on the ovaries, and in fact a periodic effort at reproduction. Although his theory was based on hypothetic notions, and interwoven with some erroneous views, still its near approach to the truth reflects considerable credit on his sagacity and discernment. His Essays on the Female Economy were published in 1821, years before the discovery of the human ovum by Baer.

The only disorder of menstruation, noticed by Dr. Meigs, is amenorrhœa. His remarks, though mixed up with some rather extravagant physiological speculations, are yet sound and judicious as regards the management of this complaint. Of ferruginous medicines, he particularly recommends an impalpable powder of the pure metal, prepared at Paris, by M. Quésnesville, the successor of MM. Pelletier and Caventou (*a*).

Chapter VIII. is headed "Pregnancy," and treats of divers matters, including the anatomy and physiology of the gravid uterus, the development of the embryo, monsters, the duration of gestation, abortion, prolapse and retroversion of the womb, some of the diseases of pregnancy, extra-uterine gestation, the signs of pregnancy, &c. Of this great variety of subjects, thus confusedly lumped together, we shall only glance at a few. With Seiler and Velpeau, Dr. Meigs rejects the Hunterian doctrine, that there is a uterine portion of the placenta, and cannot bring himself to believe that any direct vascular communication exists between this organ and the womb. His arguments in favour of this position, if closely looked into, will be found to possess but little depth or weight. In justification of his dissent from the doctrine of Hunter, he further appeals to some apparently very superficial examinations of gravid uteri, in which he was unable to detect the existence of a single vessel passing from the womb to the placenta, the "mucous tractus"(?) having been the only thing broken on separating the two. He seems to regard the facility with which this divulsion can be effected, as going far to prove the absence of utero-placental canals. Notwithstanding this avowal of his objections, he confesses, with the candour that

(*a*) A formula for its preparation is given in the second edition of Dr. Neligan's Treatise on Medicines, p. 396.

bespeaks a truly philosophic mind, that there is a strong argument against the opinions he has adopted, and, conceiving it to be his duty to state it, he does so in the following words :

"The lining membrane of both veins and arteries, which Bichat calls *membrane commune*, and which by Burdach is denominated endangium, is the true blood-vessel, the fibrous and elastic coats being mere additamenta and fortifications of the genuine blood-vessel. In the depths or interior of the viscera, the endangium is applied directly to the gangue of the viscus, which gives to venous sinuses the appearance of mere cells or cavities, particularly in the womb, where they are in some instances as large as a finger. It is a faculty of blood in motion to make a channel or vessel for itself, which vessel is found to be lined with endangium. As the womb expands in pregnancy, the orifices of vessels upon its inner wall become more and more capacious, and possibly the endangium of some of these orifices may be extended, forced, driven, or deeply impressed among the delicate spongioles and tufts of the chorion, giving rise to the appearance of vascular cylinders plunging into the placenta, or passing from it to the womb, and thus uniting the two surfaces, uterine and placental."—p. 179.

In the "argument" above stated, the intelligent reader cannot fail to recognise the peculiar theory of Dr. Reid, which is fully noticed in the review of his physiological labours in our present Number, and the omission of whose name throughout the chapter, or of any reference to his valuable researches, seems to us most unaccountable.

Dr. Meigs is a believer in protracted gestation, and receives with implicit faith the narration of Professor Asdrubali, concerning the signora "who carried twins in the womb over thirteen months." Our author gives the history of a case that fell under his own observation in hospital practice, in which pregnancy was supposed to have "endured nearly fourteen months"!

The remarks on abortion, though very brief, are replete with practical instruction. After describing Dewees' hook, and Dr. Bond's forceps for extracting dead ova, our author gives the following sound piece of advice:

"While I lay before the student these instruments for the extraction of the dead ovum, I ought to warn him against too facile a disposition as to the employment of them, and to assure him they will often disappoint his expectations, and sometimes, where they do succeed, lead to evil consequences as to the mother."—p. 215.

The consideration of the signs and symptoms of pregnancy concludes the chapter; and, allowing for the space taken up by

two cases, this important subject is dismissed in little better than four pages, but the student is referred for full information to the author's "Letters to his Class"(*a*). He enjoins great caution in the administration of emmenagogues to patients with amenorrhœa, in whom the signs of disordered health are absent, lest the unpardonable error be committed of provoking an abortion instead of removing a morbid obstruction of the catamenia. But he further warns the beginner to take special care, in his diagnosis, that he shall first know the woman to be pregnant before he dare venture to pronounce her so. These are golden precepts, and, if always acted on, would preserve the practitioner from many sad mistakes. With the exception of the sounds of the fœtal heart, he seems to have little or no confidence in any of the various signs and symptoms of pregnancy, singly or collectively. Of the placental soufflet he states that he "has not the least confidence in it as an object in obstetric auscultation."—p. 240. Even the ballottement, or "tilting the embryo upon the tip of the finger," does not furnish to his sceptical mind "any sure evidence that the tilted body is an embryo."—p. 239. In common with most accoucheurs, we have always regarded this as a decisive proof of the existence of pregnancy; and, despite the authority of the Philadelphian professor, we must continue of the same opinion till some *facts* be brought forward to shake our reliance on its value.

Dr. Meigs commences the third part of his work with some general observations relating to the character and deportment of the accoucheur. These strike us as being so very truthful and judicious that in justice to the author we partly quote them:

"In coming now to this third division of our subject, or midwifery proper, the student ought to be informed that the practice of this art is one requiring not only a large amount of obstetrical or scientific information, but also a great deal of prudence and delicacy, as well as knowledge of the world, without which he will scarcely attain to any considerable eminence or happiness in the practice of it.

"Again, so delicate and difficult, in point of conduct and conversation, are the duties of the accoucheur, that there is nothing short of absolute good sense and tact can carry a man without reproach or misfortune through a long career of this business."—p. 244.

Although Dr. Meigs does not professedly adhere to any particular method of classification, he follows in the disposition

(*a*) Reviewed in the Number of our Journal for August, 1848.

of his subject the arrangement adopted by most of the older authors, dividing labours into two great classes, whereof one includes all those we would call "natural labours," as well as the cases of pelvic presentation; whilst to the second class, which he designates "preternatural labours," he refers every case "that cannot be brought to a safe conclusion by the natural powers of the system," or "which, from some complication, whether of disease or accident, exposes the mother after delivery to the most imminent danger of death." This latter class, it will be observed, is very comprehensive, embracing not only one division of the preternatural, and all the difficult and anomalous cases of Denman, but every labour complicated by any disease whose presence at this particular period may endanger life.

Respecting the use of the lancet in labour our author gives very prudent counsel. He is no advocate for the copious depletion so much practised and so highly lauded by his countryman, Dr. Dewees; on the contrary, he discountenances large bleedings, or even beyond sixteen ounces, on ordinary occasions, and with this our own experience fully accords. We feel compelled to dissent from him, when he advises the "powerful resource" of introducing the hand into the vagina or uterus, for the purpose of altering the position of the head, in every case where it is not descending as favourably as might be wished. Such practice, we think, may very justly come under the denomination of "meddlesome midwifery," which Dr. Meigs himself so justly deprecates, and cautions the student against. His views upon the nature of hour-glass contraction of the uterus are confirmatory of the doctrine first promulgated by Dr. Douglas, of this city, namely, that this is never a primary cause of retention of the after-birth, but consequent upon morbid adhesion(*a*). So strongly impressed is Dr. Meigs with the truth of this doctrine, that he "heartily and warmly" advises the student "to introduce his hand to separate the placenta, as soon as he can clearly determine that the real hour-glass contraction does exist."—p. 393.

Some pages are devoted to etherization, the subject being introduced in the following emphatic words:

"In speaking of the various points in the conduct of a labour, I cannot well eschew to say something upon the employment of

(*a*) Dr. Douglas's paper was first published in the sixth volume of the Transactions of the London College of Physicians, 1820.

those anæsthetic agents, whose recent irruption into the domain of medicine and surgery has been so sudden, violent, and overbearing." —p. 315.

He publishes Dr. Simpson's letter to him, and his own reply, with the contents of which our readers must be already familiar. Dr. Meigs adheres firmly to the opinions he expressed in this celebrated letter, but admits, as every unprejudiced man must, "that there may be instances of severe suffering for women in labour, that ought to be mitigated, or even wholly obviated, by casting the woman into the profound anæsthesia of chloroformization."—p. 316. He omits to notice two serious fallacies that pervade the writings of all the more strenuous advocates for anæsthesia in midwifery, and which form the basis of many of their arguments for its general employment. The first of these is, their statement that the pain of labour is analogous, as regards its effects, to that produced by disease or the surgeon's knife; whereas, to use the words of Dr. Montgomery, "no two things having a partial resemblance can be more essentially dissimilar;" the one being a healthy, natural, physiological pain, and the other a morbid, unnatural, pathological pain. The second grand error into which these ardent defenders of the Lethean practice have fallen, is their asserting that pain is the destructive agent in protracted labours; the truth of which proposition may be very fairly impugned, since experience amply shows that the pain is only a subordinate element of danger, and that the effects of tedious labour most to be dreaded arise from the long-continued muscular exertion,—which, as Dr. Simpson has explained, was implied in the primæval curse,—and the severe pressure and contusion sustained by the soft parts, causing fever, local congestion, inflammation, and gangrene(*a*).

The twelfth chapter contains remarks upon presentation of the pelvic extremity of the fœtus. Dr. Meigs' directions for the management of these cases are most ample, but he greatly exaggerates the frequency with which recourse to the forceps is advisable in order to complete the delivery. No practitioner in these countries ever dreams, we believe, of "ordering a forceps to be got in readiness as soon as he ascertains that the presentation is not one of the head;" and yet such is Dr. Meigs' "unfailing custom." We can only just conceive a case in which

(*a*) Dr. Montgomery's able and scholarlike paper on this subject appeared in our Number for May, and should be perused by every one interested in the question of obstetric anæsthesia.

they might be required, never having seen an instance out of the hundreds that have come under our observation. The causes of pelvic presentations he thinks must be purely accidental, and adds, "the natural presentation is that of the head, which is turned towards the os uteri from the earliest period of pregnancy."—p. 345. The same opinion he also expresses in two other places (pp. 67 and 184). On this point we differ from the Professor, and our reasons for doing so we shall briefly state. The position of the fœtus *in utero* during the successive periods of gestation is a subject that has only of late years attracted close attention; there is abundant evidence, however, to warrant the conclusion that up to the seventh or eighth month the pelvic and cephalic extremities of the fœtal ovum present at the os with nearly equal frequency. Thus, according to M. P. Dubois, the frequency of presentations of the lower extremities is found to increase in proportion as the expulsion of the fœtus takes place near the early periods of pregnancy; and in Drs. M'Clintock and Hardy's Practical Observations on Midwifery it is stated that, out of 108 premature children, twenty-nine presented with the lower extremities; whilst of twenty-seven premature births, the particulars of which are now before us, the feet or breech presented in eleven.

Dr. Meigs' description of the process of spontaneous evolution coincides in all important particulars with that of Dr. J. C. Douglas.

Of all the complications of labour, there is none which the accoucheur should be more thoroughly acquainted with than hemorrhage; and accordingly we find Dr. Meigs devoting many pages to its consideration. His disbelief in the direct and intimate vascular union of the placenta and uterus has been already mentioned; and now, in order to account for the enormous sanguineous discharges which the gravid womb is capable of yielding, he broaches an opinion which, how ingenious soever it may be, is nevertheless wholly unsupported by so much as a shadow of proof. He writes:

> "When blood escapes from the uterus, it must be, I think, in consequence of a hemorrhagic nisus or sanguine determination, like that which sometimes causes the effusion of blood from the Schneiderian membrane, in those cases of epistaxis that come on spontaneously."—p. 377.

Upon the means of diagnosing between accidental and unavoidable hemorrhage he gives very scanty information; and yet this is a question of primary importance, and oftentimes most difficult of solution. The plan of detaching the placenta

in cases where its insertion over the os is the cause of flooding,—a practice suggested so long back as the year 1799, by Dr. Chapman,—receives no countenance from Dr. Meigs, as will be seen from the following passages:—

"To say that the detachment of the placenta, without any consequent reduction of the superficial contents of the uterus, could arrest a hemorrhage by breaking off the curling arteries (as they are called) of the womb, appears to me quite unphilosophical, for there are thousands of facts of ante-partum and post-partum hemorrhages to prove that the arrest of hemorrhage is the consequence of condensation of the womb under its muscular contraction."—p. 388.

"I cannot but think that, in any case in which it is possible to detach the whole of the placenta, it would be also possible to introduce the whole of the hand, and thus commence at once the operation of turning, which ought to be esteemed as the essential indication of treatment in placenta prævia, and which, the earlier it is done, so much the greater chance does it give, both of rescuing the child and saving the woman from fatal losses of blood."—p. 389.

He barely alludes to the stimulant and restorative measures required to support the system under the exorbitant demands made upon it in cases of severe flooding; but he insists strongly on the necessity of confining the patient to a horizontal position for some time afterwards, and alleges that during the deliquium so apt to be induced by a neglect of this precaution, the formation of false polypus in the cavities of the heart is very likely to take place. It is not without considerable surprise we find Dr. Meigs recommending the exhibition of vitriolic acid in infusion of roses, acetate of lead combined with opium, &c., for post-partum hemorrhage. As well might one try to extinguish a conflagration with a penny syringe, as to expect that such means could be of any—the slightest—utility in controlling uterine hemorrhage at the full term of pregnancy; or, to use a favourite illustration of the late Professor Hamilton of Edinburgh, it would be just as rational practice to try and stop the bleeding from a cut finger by the administration of internal astringents.

Dr. Meigs entertains the most profound dread of small-pox attacking a pregnant woman, observing that, "if she be confined or suffer abortion, she shall hardly escape death by hemorrhage from the womb, or by metro-phlebitis, coming on early after the detachment and expulsion of the placenta."—p. 431. To such an extent is he imbued with this variola-phobia that we find the following stringent precautions enjoined:

"Now let the student beware not to expose his gravid patient

to the least danger of variolous infection; and, therefore, let him never dare, under any circumstances, to vaccinate a pregnant woman, or one recently confined." . . . . . "I reiterate the assertion that the virus of small-pox is eminently inimical to the life of the pregnant female; and I aver that the virus of the vaccine inoculation is little less so than unmitigated small-pox." . . . . . "I am firmly convinced that it is far better for the physician, during the epidemic of small-pox, to leave his pregnant patient to the chance of a natural infection, than certainly to bring her within the scope of its virulent power by a vaccine inoculation, which is but a variolous inoculation modified by the generical force of an inferior zoological genus." —p. 432.

It would be a mere waste of time, we believe, to enter into any serious refutation of the above statements regarding the danger of vaccine inoculation to pregnant females. We do no more, therefore, than express our astonishment at the opinion, and our anxious wish to know the facts upon which it is founded.

We heartily concur in the author's sensible remarks upon the practice of turning in cases of deformed pelvis. The objection he advances is one that has always appeared to us to possess great weight. He writes:—

"I am not prepared in this place to go so far as Professor Simpson of Edinburgh, in recommending a resort to version in bad pelves; and my hesitation arises from this, that the mensuration of the pelvis, being rather a non-dependable process, there is reason to fear that the incautious and inexperienced accoucheur might, by too free a recommendation of it, be led to institute this method in cases where the reduction of the diameters is so considerable as to render an excerebration indispensable for the delivery."—p. 458.

Laceration of the womb and vagina our author justly describes as "the most terrible accident which can happen to women in labour;" yet the subject is dismissed in little better than two pages, and even this limited space is unprofitably occupied. Upwards of forty pages are devoted to the history and uses of the forceps, and we regret that we cannot afford space to give a particular analysis of this chapter. We can only say that, where the child is alive, Dr. Meigs appears to set almost no bounds to the applicability of the instrument. Even in cases of regular impaction, where the head is so tightly jammed into the pelvis "that the forceps can find no space in which to pass," we find him, paradoxical though it may seem, urging that the attempt should be made. Such unqualified recommendation of the forceps, from one in Dr. Meigs' authoritative position, is much to be deplored; as it is calculated to make this instrument—so pre-eminently service-

able when rightly used—an agent of destruction, as well as a source of disappointment and professional obloquy. These strictures we consider to be the more necessary, as Dr. Meigs tells us in his dedicatory letter that this book he has "addressed to the student, and that to him he speaks in every page." In place of the crotchet, which he pronounces a "detestable instrument," he uses two pair of forceps or pliers, with long handles, for extracting the head. The induction of premature labour Dr. Meigs thinks to be legitimate under certain circumstances; but it is apparently with extreme caution and reserve that he speaks of this operation, nor does he give any directions as to its performance.

Inversion of the uterus is the subject of the eighteenth chapter, and upon the acute form of this disease he makes some excellent practical observations. The prominent feature of novelty, however, is the announcement of his "opinion that there are instances of inversio uteri, in which, by means of some power, the processes of which are at present not known or understood, the womb reposits itself."—p. 555. In support of this he relates three cases, which, together with his remarks thereon, well deserve the attention of obstetricians.

With regard to puerperal fever our author declares himself a decided non-contagionist, and will only admit one species of the disease—the inflammatory, for the cure of which the bold and repeated use of the lancet is the invariable resource. Notwithstanding these erroneously exclusive views (in which he strictly follows Gordon), the article in question contains many valuable hints and observations upon this one form of puerperal fever. The truthfulness of the following picture must be acknowledged by every one:

"A woman lies down on the *lit de misere* in order that she may give birth to a child; an attack of puerperal fever too often converts it into her bed of death. A man goes to his bed in fever under the apprehension of approaching death; he is RESCUED by the physician, but the accouchée who perishes is LOST. There is a great difference in the sentiment connected with the two."—p. 581.

Dr. Meigs concludes this chapter with some extracts from a letter to him from Dr. Collins, in which this gentleman clearly demonstrates that the "vehement antiphlogistic method" of Dr. Meigs, in the treatment of puerperal fever, was wholly inapplicable to the majority of cases under his care in the Dublin Lying-in Hospital.

In the fourth part of the work Dr. Meigs gives most admirable directions as to the food, clothing, and general manage-

ment of new-born infants, as well as for the treatment of the more common diseases observable in this early stage of existence. Cyanosis neonatorum is treated of at great length, but his observations chiefly refer to one form of this affection, viz., that in which the foramen ovale remains patent after birth. For the treatment of this he confidently recommends the observance of a nearly horizontal decubitus on the right side, in which position of the body he supposes the blood of the left auricle will, in obedience to the law of gravity, press upon and close down the valve of Botal. He cites many examples from his own, as well as from the experience of others, in which this simple mode of treatment was followed by a permanent cure, or marked temporary relief.

It is with no small regret we have been obliged to notice many chapters of this volume in a very rapid manner, and to pass others over altogether. Amongst the latter are to be found some highly valuable remarks upon deformed pelvis, atresia vaginæ, ergot of rye, milk fever, &c. &c.

As the impression on the mind after reading a work is in some measure influenced by the opinion formed of its object and character, it is not unjust or unkind to say that Dr. Meigs' book has little pretensions to being a complete systematic treatise; and that whoever reads it with any such expectation will be grievously disappointed by its want of arrangement, by the omission of many important subjects, and by the little reference to the writings of contemporary or standard authors. But, on the other hand, if it be viewed as a record of the experience and observations of one who has passed many years in extensive practice, and is gifted with a more than ordinary amount of discernment and talent, we are convinced that its attentive perusal will more than repay the reader, by affording him both pleasure and instruction.

On many questions of considerable moment our opinions do not agree with those of Dr. Meigs; and in some points of practice we even regard his precepts as evincing a reprehensible fondness for artificial interference, as well as a want of confidence in the powers of nature to remedy or overcome comparatively trifling obstacles. Notwithstanding these differences, however, we are free to confess that we read this volume with much interest and gratification. We admire the spirit in which the author writes; his style, too, has an air of unrestrained freedom and novelty about it that is very fascinating; and on many topics he supplies information not to be found elsewhere. The wood-cuts and diagrams are at least effective, if not very artistic; and of the numerous cases introduced it is not too much

to say that they are apposite to the subject in hand, and are related in a clear, impressive manner. On the whole, we consider Dr. Meigs' book as a valuable acquisition to obstetric literature, and one that will very much assist the practitioner under many circumstances of doubt and perplexity.

On the Diseases of Infants and Children. By Fleetwood Churchill, M. D., M. R. I. A., &c. &c. Dublin, Hodges and Smith. 8vo. pp. 650.

The period is not very remote since the diseases of infancy and childhood began to engage the distinct attention of physicians, and to form the subject of special treatises; and with such perseverance and success has this department of medicine been cultivated, that from, comparatively speaking, a mere point, it has expanded into a wide field of knowledge and pathological inquiry. Within the last few years an immense number of excellent monographs, bearing reference solely to the maladies of early life, have appeared in England, France, Germany, and America. These have become so numerous, and the amount of original matter which they contain so large, as to render a well digested treatise, exhibiting the opinions and practice of leading authors, a great desideratum at the present time. Now, we think that the work before us is eminently calculated to supply this want in our medical literature; and after an attentive perusal of its contents, we hesitate not to say, that it is one of the most comprehensive ever written upon the diseases of children, and that, for copiousness of reference, extent of research, and perspicuity of detail, it is scarcely to be equalled, and not to be excelled, in any language.

From Dr. Churchill's known ability and industry, we were led to form high expectations of this work; nor were we deceived. Its learned author seems to have set no bounds to his researches in collecting information, which, with his usual systematic address, he has disposed of in the most clear and concise manner, so as to lay before the render every opinion of importance bearing upon the subject under consideration.

The first part of this work, embracing five chapters, chiefly relates to the hygiene of infantile life. Our author, in the "preliminary observations," adduces statistics, drawn from various authentic sources, to show the high rate of mortality during childhood and infancy, and he succinctly points out the causes of disease to which the infant is exposed before, at, or immediately after its entrance into life. In the four subsequent chapters he briefly but

clearly lays down the principles which should guide us in regulating the food, clothing, and general management of infants. The observations on each of these heads are just what we should have expected them to be,—sound and judicious. This testimony we freely give, even though there may be one or two points on which we would differ with the learned author. Thus we think that the custom of giving aperient medicine to every newborn babe is open to serious objection ; no doubt many will require it, and others will be nothing the worse of it, but some will sustain positive injury from this rude forcing of natural processes. The ingestion of food will generally stimulate the bowels to act within ten or twelve hours; but if this time elapse without an evacuation, then a small dose of castor oil, as recommended by Dr. Churchill, may be given with propriety and advantage. Our author does not sanction the present fashion of infants going in their bare heads; " experience," he observes, " has proved the folly of this attempt at braving natural laws to gratify a theoretical prejudice. In this and similar ways, I dare say, a *race* may be hardened, but it is by cutting off the weaker members."—p. 39.

The second part of the work commences in Section I. with the diseases of the cerebro-spinal system. The plan pursued by the author in each section of the present division of the volume is, first, to notice the diseases by which the fœtus is most frequently attacked during intra-uterine life; then to treat of those which it presents to us at birth, whether of long-standing or acquired during parturition, together with certain malformations which require treatment; and lastly, to enter at length into the consideration of the diseases which affect infancy and childhood. This is an excellent arrangement, and very convenient for purposes of reference. Having described the lesions of which the nervous centres may be the seat before or at birth, he makes some admirable remarks on " irritation of the nervous system," particularly noticing the two opposite states of the constitution under which it may present itself. A clear and perfect comprehension of this condition will materially assist the practitioner in his medical intercourse with children, and will oftentimes enable him to ward off impending danger, or induce him to abstain from needlessly active measures. Dr. Churchill is unable from his own experience to throw any light upon that obscure and enigmatical affection, " trismus nascentium." To the long list of remedies he has mentioned as having been fruitlessly tried in this disorder, may be added Indian hemp. In a limited number of cases we have tried the tincture of the resin, prepared by Mr. Donovan, but it merely seemed to retard

the progress of the disease, and perhaps to mitigate the severity of the fits, without lessening the certainty of a fatal issue.

Amongst the diseases incident to early life, Convulsions, though strictly but a symptom, must ever hold a prominent place, being an occurrence no less frequent than formidable. As the grand question for solution in all cases of the kind, is to ascertain the cause of the attack, we find that nearly every division has this for its basis. Dr. Churchill makes three forms or varieties, viz.:

"1. Primary or essential convulsions, arising from irritation of various kinds, mental emotion, &c.; 2. Sympathetic convulsions occurring in the course of fevers, or organic diseases of any kind, excepting those of the brain or spinal marrow; and 3. Symptomatic convulsions, connected with diseases of the head or spine."—p. 97.

Though perhaps not the most scientific, this is, nevertheless, a good classification for practical purposes; and, in order to illustrate its application, we quote the following highly instructive comments:

"Suppose we are called to a child from one to six years old, strong and hitherto healthy, who has had a convulsion following a fright, blow, fall, indigestion, &c. The convulsion may of course be primary, sympathetic, or symptomatic, but upon further inquiry we find that the child was perfectly well up to the moment of the attack, that the exciting cause is plain, the constitution sound, the access not very violent, and that there are no other head symptoms. So far, then, the case is one of primary or sympathetic convulsions; but further investigation proves that there is no disease of the chest, abdomen, &c., and we conclude that the convulsions are primary or essential. But if, on examination, we discover evidences of pectoral or abdominal disease, acute or of long standing, we must then infer that the convulsions are sympathetic, and we cannot be too minute in our examinations of all the organs in every case, as the treatment as well as the diagnosis will depend upon it. In these primary and sympathetic attacks the brain and nervous system are in a state of sympathetic irritation only in most cases, but we cannot be quite sure of this when the fit comes on in the course of some chronic disease, as, for instance, in tuberculous affections, in which it is quite probable that the brain may be the seat of a similar deposit. Such cases render the diagnosis very difficult."—p. 107.

The subject of Hydrocephalus is treated with that minuteness of detail which its very great importance so justly claims. Without taking morbid anatomy solely for his guide, and at the same time without entirely discarding it, our author describes four species or phases of the disease. The *first* corresponds to the acute hydrocephalus of Gölis and others, and to

the "meningite simple aigue" of Barrier, and Rilliet and Barthez; the *second* form, corresponding to Cheyne's first species, is more deliberate in its commencement and progress, and very often occurs as a secondary affection; the *third* form is the tubercular meningitis of French authors, and in point of frequency should hold first place; and the *fourth* form is the water-stroke of Gölis and the Germans, and the apoplexia hydrocephalica of Cullen. Under each of these heads is given a very accurate portraiture of the characteristic features and course of the complaint; indeed, we may truly say that in everything relating to the symptoms, diagnosis, pathology, treatment, and prophylaxis of water on the brain, this chapter is replete with information, and is in itself a compendious exposition of the existing state of knowledge upon this disease. Chronic hydrocephalus, encephalitis, apoplexy, and the other lesions of the nervous system, are handled in the same satisfactory manner.

The chapter on Laryngismus Stridulus, or, to use the more simple and explicit term of Sir Henry Marsh, spasm of the glottis, is an able and instructive one. As the pathology of this affection has been a matter of dispute between some justly respected authorities, Dr. Churchill fully investigates this question, stating the different opinions entertained, and the grounds on which they are based. His testimony is against the doctrine which imputes the disease to enlargement of the thymus or bronchial glands, and in this conclusion he is strongly supported by many writers, and especially by Dr. James Reid, the latest author on the subject. Dr. Churchill has found benefit from the use of a small seton in some cases, which accords with our own experience:

> "It will be advisable," he remarks, "in most cases, to apply some form of counter-irritation, either some irritating liniment, or a blister, or, what I have found far better, a small seton in the arm. In one case under my care, the moment the seton discharged fairly, the spasms ceased, and whenever it was left out they returned."—p. 196.

He strongly insists on close attention to the diet of children who are liable to laryngeal spasm, and also on the necessity of pure mild air; and certainly these are circumstances whose importance cannot be overrated in the management of the complaint.

Our author only recognises two stages in Pertussis, the catarrhal and spasmodic, which is the simplest and most natural division of the disease. The physical signs audible during the paroxysm

of hooping-cough have been described by Laennec, Williams, and others; but none of these stethoscopists, we believe, have assigned any peculiar character to the respiratory sounds in the interval between the fits of coughing. The result of several carefully conducted examinations leads Dr. Churchill, however, to believe that the murmur of inspiration and expiration does present at these times a certain character quite peculiar to pertussis. As this is a point of considerable consequence and novelty, we must, in justice to Dr. Churchill, quote his own words:

"I have examined a great many children at intervals from one paroxysm to another, and I have in a great many cases found that, after the chest had been cleared by the last cough and vomiting, the respiratory murmur or inspiration was louder and more rough than usual, nay, in some cases, it had a rather loud, brazen sound, something resembling a loud sonorous râle, as if the air was passing through tubes much narrower than usual. It is perceptible, also, in expiration, though more feeble. This sound may continue until the mucus begins again to accumulate, and then it will be exchanged for the large mucous, bubbling sound, which increases until the next cough, and is almost universal. In milder cases the rough, dry sound is more feeble, though generally audible, and I think this loud, rough murmur of inspiration and expiration quite peculiar to pertussis. The chest is clear on percussion throughout this stage in simple cases."—p. 203.

It remains for subsequent experience to establish or overturn these conclusions. Should Dr. Churchill's observations be found correct, then we shall be in possession of a most valuable diagnostic of pertussis, and one, too, which will be available at those periods in which we have, at present, no means of positively diagnosing the disease. For mitigating or suspending the spasm in hooping-cough, Dr. Churchill has tried the inhalation of sulphuric ether at the commencement of the fit, and in most of the cases with marked success. Chloroform he has also employed, and with a like result. In two-thirds of the cases the course of the disease was decidedly shortened by this treatment, and in no instance was insensibility or the least inconvenience occasioned. As the complications of hooping-cough are the sources of greatest danger and difficulty in its management, Dr. Churchill devotes much space to their consideration, and his remarks on each of them abound in wisdom and practical instruction.

Chapter IV. contains a masterly treatise on Croup, and displays the most patient investigation, and the most calm, truthful collation of facts. That much-disputed question as to the

propriety of laryngotomy in this disease, receives full and impartial consideration, and a great mass of evidence is adduced on both sides. He concludes his review of the arguments for and against the operation with the following summary of the objections to its employment, to each of which we feel bound to subscribe:

"1. That the larynx is not mechanically closed by false membrane; that in all cases, as Dr. Cheyne has remarked, there is sufficient space for the access of air; that if the larynx be closed it must be by spasm in addition to the exudation; and that, therefore, to attempt relief by a mechanical operation would be superfluous, to say the least of it.

"2. That it is extremely difficult to say that exudation has taken place, and still more to fix the limits of it, and pronounce in any case that it has not extended below the larynx; and yet upon this depends the utility of the operation; for,

"3. If the false membranes have extended below our incision, the operation, being purely mechanical, can afford no relief, but may seriously add to the danger.

"4. Bronchitis or pneumonia may exist at the time of the operation, or may very likely arise very soon after, and render it altogether useless.

"5. The operation itself is not without danger, nor quite so easy as has been stated, especially with young infants. In addition to hæmorrhage and escape of blood into the trachea, the patient may be attacked by prolonged syncope, asphyxia, or convulsions, as occurred in M. Trousseau's practice, and occasionally either of them may prove fatal.

"6. That the risk of inflammation and other accidents after the operation is very considerable, and materially diminishes its value.

"7. That the results of the operation hitherto, although successful to a considerable extent, are not sufficient to justify our having recourse to it under ordinary circumstances. 'If,' says Mr. Porter, 'it were possible to place a host of those cases in which bronchotomy had not proved serviceable, in array against those wherein it had seemed to be useful, it would scarcely be necessary to advance any further argument in proof of its uncertainty.'"—p. 259.

We lament being compelled thus briefly to notice this valuable article; not less, however, do we think it deserving of special attention. Dr. Churchill does not allude to the topical application of solution of nitrate of silver in croup; but we believe, when he wrote his fourth chapter, Dr. Green's book on that subject had not appeared in this country. The three remaining chapters of this section are upon bronchitis, pneumonia, and pleurisy. Like the preceding ones, they are rich in information and full in detail, leaving no point of importance or

interest unnoticed. When speaking of pericarditis, Dr. Churchill strongly recommends to his readers a rule he has adopted for many years, and one whose value we can fully attest:

"When first called to see a child, no matter for what disease, to examine every organ of the body, and to repeat this examination at intervals of a few days. By so doing we shall often ascertain the commencement of secondary affections before they give rise to any complaint of distress."—p. 339.

Of the congenital malformations of the digestive system, hare-lip, cleft palate, and imperforate anus, are described at length, with their respective modes of treatment. Then follows a chapter on Dentition, from which we quote one or two observations about that lightly esteemed operation of lancing the gums, which, we are satisfied, is more often undertaken than rightly understood or performed:

"A slight scarification of the gums, for the relief of teething, is of no use whatever; they must be cut down until we feel the lancet touch the tooth, and to the full extent of the swollen gum, and a little further. I have often seen the irritation continue as severe as ever after an incision over the central incisors, because the operator had not noticed that the lateral ones were pressing forward, and so of the other teeth."

"And I would beg to impress on the student that, owing to the fright of the mother or nurse, and the cries and resistance of the child, to lance the gums effectually is by no means an easy operation, but one that requires both firmness and deliberation to avoid, on the one hand, cutting too superficially or too limitedly, and, on the other, wounding the mouth or tongue."—p. 375.

Upon the various diseases of the mouth and pharynx in children, Dr. Churchill has brought together a great amount of information. Besides all the forms of stomatitis, he minutely describes the symptoms and treatment of tonsillitis, cynanche parotidea, diphtheritis, putrid sore throat, and abscess behind the œsophagus.

Diarrhœa and Dysentery are very fully considered: of the former, our author enumerates five different varieties, making the character of the discharges the basis of the division, viz., the feculent, bilious, mucous, chylous, and lienteric diarrhœa. Objection may perhaps be taken to the term *chylous* diarrhœa, since it is still a disputed question whether it really is chyle which passes off in these cases. For the treatment of diarrhœa, which so often well nigh exhausts the patience and skill of the practitioner by its obstinacy, Dr. Churchill enumerates a great variety of remedies, some of which are but little known in this

country, and well deserve the attention of physicians. He very properly devotes a chapter to "Helminthiasis," or verminous complaints of children, although he frankly confesses his disbelief in the existence of any symptoms pathognomic of worms, and agrees with Dr. West that the only proof of worms being present is seeing them. In this conclusion we cannot fully concur, and yet we think he hardly exaggerates when he says: "Even at the present day, any disease whose nature is not very clear, any symptom of disorder of the digestive system, or of general nutrition, which is obscure, is solved by the magical abracadabra of worms." To the ordinary list of anthelmintics he adds several others whose efficacy is attested by high authorities; so that the practitioner is here furnished with many resources against these entozoa.

Diseases of the liver and spleen form the subject of the twentieth chapter, which may be said to contain the substance of all that is at present known of these infantile complaints. Dr. Battersby's excellent and original monograph, which appeared in the fourteenth Number of this Journal, is done ample justice to in this article. Dr. Churchill's observations upon Chronic Peritonitis merit particular attention. To the usual modes of treatment this not generally recognised disease is little amenable, so that any other plan holding out a prospect of better success is deserving of every attention. After reciting the usual remedies, he proceeds to observe:—

"But our principal reliance is upon mercury, given so as to affect the gums, if possible. I believe that the credit of thus administering mercury in this disease is due to Sir Henry Marsh, as I have found no allusion to it in any authority. It may be exhibited internally or by inunction; in many cases the latter is preferable, as, when diarrhœa occurs, the bowels are too irritable. A scruple of the strong ung. hyd. should be gently rubbed in over the abdomen, night and morning, and continued until the gums are touched, or the disease shows signs of yielding to the treatment."—p. 524.

The fifth section, at which we have now arrived, contains a short sketch of those cutaneous diseases which occur most frequently in children, and as the observations thereon make no pretensions to originality, we may pass them over without further comment, merely remarking that here, as elsewhere, our author is clear and concise in his arrangement and language.

We cannot sufficiently praise the comprehensive and lucid manner in which Dr. Churchill treats of the Eruptive Fevers. Suffice it to say that in no part of the work has he evinced so much judgment and erudition. Indeed it would appear as

though this important section of the work had been the object of much labour and particular attention, and we feel convinced that the most experienced will derive instruction and profit from its perusal. We would gladly have reproduced here some of his valuable observations, but our typographical limits of circumvallation are too rigorously defined to admit of our doing so.

Our general estimate of this work may be easily gathered from what has preceded. We welcome it as a most valuable acquisition to infantile medicine, and doubt not but that ere long it will be in the hands of every practitioner. It is a matter of regret, we think, that the author did not incorporate more of his own personal experience, as such would have considerably enhanced the value of the book. Dr. Churchill's scrupulous care on all occasions to render full justice to every author, and his punctilious honesty in naming every authority whose opinion or practice may happen to coincide with his own, are alike conspicuous, and demand our warmest admiration; such conscientiousness is not, we regret to say, common at the present day. Although he has omitted to notice some diseases specially belonging to children, still we regard this volume as possessing more claims to completeness than any other of the kind with which we are acquainted. Most cordially and earnestly, therefore, do we commend it to our professional brethren, and we feel assured that the stamp of their approbation will in due time be impressed upon it.

The Microscopic Anatomy of the Human Body in Health and Disease. Illustrated with numerous Drawings in Colour. By Arthur Hill Hassall. London, Highley. 2 vols. 8vo. pp. 578.

This work is now complete in fifteen parts. We have already noticed, with approbation, some of the earlier numbers; but as we regard the publication of a compilation of the kind, in the English language, as a momentous addition to our scientific literature, we think it due to our readers to inform them more particularly of its contents, and to beseech their attention to its subject. The knowledge of life and of disease has yet to be acquired. Who can say that the opinions which have been hitherto put forward, as to the causes of normal or perverted organization, have had any real foundation in a truly logical induction? From the excitability of Glisson to the organic sensibility of Bichat,—the plus or minus vitality of Brouissais,

or the plus or minus waste of Liebig; all are deserving of little more respect than to be regarded as speculative explanations of some few and imperfectly observed phenomena. We have had as yet no Newton nor Jussieu in the philosophy of anatomy; what the one effected for dynamics and the other for natural science,—the discovery of the master chord which held the whole scale in harmony,—remains yet to be achieved for physiology. But Galileos and Keplers necessarily preceded Newton; and had there not been a Linnæus, it is probable that the law of the subordination of characters would never have been applied to the classification of living beings. We pride ourselves in Ireland on being good practical physicians and surgeons; and it is said we effect to sneer at the ultra-refinements of the microscopist and chemist; yet is it not true that we pursue diagnosis and therapeutics merely as arts?—that we are not entitled to claim for them the rank of sciences? Science has been said to be the art of predicting; and certainly the closer we draw classified knowledge towards the domain of syllogism, the more distinct and applicable will become our ideas. But to do this we must accumulate facts, not merely collective facts, such as structure and symptoms, but also, and more importantly, elementary facts, such as primitive cells and fibres, and molecular changes of combination. Kepler was laughed at for his swing-swongs by the practical men of his day, and the microscopist and chemist may be sneered at now; but by the pendulum was measured and tested the law of gravity; and it is only when a sufficient number of phenomena have been exactly observed and faithfully chronicled, that those general laws in nutrition and secretion can be arrived at, upon which philosophical physiology and therapeutics can alone be based. It is with the conviction of these truths that we regard with much pleasure the completion of Mr. Hassall's work. It places at the disposal of our young and working men the various observations made in minute human anatomy by the best micrographers in Great Britain and Germany. Hitherto this kind of knowledge was scattered through many expensive works, or hidden in the transactions of societies; and therefore to be presented with it, in a cheap and compact form, illustrated by drawings sufficiently executed, ought to be considered a great boon. We hope that our young anatomists will avail themselves of it, and speedily put themselves into a position to add to the stores of facts, now accumulating. Microscopical anatomy presents an opening to young men ambitious to distinguish themselves, the neglect of which by the students educated in our schools would occasion us regret. We have no

desire that they should in the slightest degree neglect or postpone those studies which have hitherto so eminently fitted them for the practical duties of their profession; but in the years which generally intervene between the acquisition of a diploma and the occupation of practice, some time may with advantage be devoted to the study of histological anatomy. In our review of Lebert's work, in a former volume, we insisted on the necessity of education to constitute a micrographer. The microscopical examination of a diseased structure, by one previously uninstructed in the use of the instrument, or unacquainted with the natural appearances of the healthy organs, must necessarily be deserving of little reliance; yet we fear that this has hitherto been too much the case in this country. We now repeat our recommendation to those young physicians and surgeons who are, to use a common phrase, "waiting for practice," to accustom themselves to the use of the microscope. Quecket's book will guide them in manipulation; Hassall's will teach them the microscopical appearances of the healthy tissues; and Vogel and Lebert will instruct them in the alterations produced by disease. One of Oberhauser's microscopes, with a power equal to between 500 and 600 diameters, can be obtained for about £6 sterling; and with these materials, and a few simple tests, the young medical man will be enabled to fit himself for the time that is coming, and perhaps win a name of honour for himself and country.

We shall devote a few pages to the appearances revealed to the observer by the microscope in the minute structure of the human organs; by doing so, we shall probably best excite that interest about the subject which we wish to inspire; for we have always observed that the works of nature themselves, speak more strongly to the affections and imagination, than the warmest eulogy of the commentator. We shall, therefore, proceed to exhibit, as correctly as our command of language will permit, some of the peculiarities of animal organization.

Some of the simpler vegetables are known to be composed of a homogeneous and structureless gelatine, containing cells scattered through its substance. Such is the nature of the little plants which occasionally confer a rosy colour on snow or ice; such is the nature of the gory dew, which to this day seems to sweat out of the ground on the field of Hastings. Such are those masses of jelly found amongst moss and heath, and which our peasantry believe to be *fallen stars*. According as we ascend to the examination of more complicated vegetables, we find them entirely composed of cells, each with its own especial walls, congregated together, or placed one after

another, but without any of that investing structureless basis which constitutes the chief part of the plants before alluded to. Now it is remarkable, that the cartilages are the only constituents of the animal framework which at all resemble the structure of vegetables; but the plants which they are like are not the highly developed cellular and vascular vegetables, but it is in the simpler jelly-like algæ that we find their types. Like the latter, they are composed of a homogeneous structureless base, in which, although fibres can be sometimes found, they are plainly a superadded element, and in which no cells can be discovered. Disseminated through this basis, there are numerous cavities in which cells are contained, very similar in structure to the cells of protococcus, palmella, or nostoc, amongst plants.

With the exception of the cartilages, all the other tissues are formed of fibres. These fibres, in every instance, are produced, there is reason to believe, by the development of primitive nucleated cells; these cells become lengthened at opposite extremities, and at last become prolonged into long fibres. This happens as well in the so-called cellular tissue as in the tendons and fasciæ. The areolar tissue, which has been called "cellular" in animals, is not entitled to that appellation in the same sense in which it is applied to the cellular tissue of vegetables. The latter is indeed composed of cells or utricles, little closed sacs applied against each other, and, if not very closely packed, leaving minute passages between them,—the intercellular passages. But the areolar tissue of animals consists of plates crossing each other at various angles, and thus forming cavities which freely communicate with each other; but one of these cavities cannot anatomically be called a cell, for it is really only an interspace among the true anatomical elements. These elements are fibres which, lying beside each other, and coiled so as to present an appearance, when magnified, something similar to a skein of thread allowed to fall on a table, constitute collectively the plates by whose disposition the areolar tissue is constructed. We, in this instance, perceive that animal and vegetable structure is essentially dissimilar.

Vegetable cells, for the most part, soon lose their nuclei. This is not the case with animal fibres. The primitive cells become altered and transformed into long, hollow threads, closed at both extremities, but the nuclei persist without apparent change. This constitutes another marked difference between animal and vegetable organization.

There are, however, cells formed by the animal organism, which are not destined to undergo fibrous transformation. It

is remarkable, in accordance with the view of animal structures given, that those cells that do not change into fibres are always thrown off from the system, either in the form of epithelium or cuticle, or in the secretion of conglomerate or simple glands; and in the few instances in which analogous products are destined to maintain a temporary, although fugitive, connexion with the organism, as in the case of hairs, the cells take on a tendency to fibrous development; the exception thus proving the rule.

The fibres which enter into the composition of the animal framework are of various kinds. Perhaps their physical characters present the easiest mode of classifying them. They may be divided into the inelastic, the elastic, and the contractile. The inelastic are to be found in areolar tissue, the osseous fabric, and fibrous structures, such as fasciæ, tendons, ligaments, &c. The elastic are present in the yellow ligaments and elastic cartilages; a particular variety is also present in the intervertebral cartilages. The contractile are either non-striated and separated, or striated, and bound into bundles by meshes of inelastic fibres (sarcolemma). In the former case they constitute the involuntary, in the latter, the voluntary muscles. These contractile fibres, however, although presenting different appearances to the eye, are essentially of the same nature, as may be proved by treating the striated fibres of the heart with acetic acid, when the striæ will be found to disappear. It is indeed by chemical means that the different kinds of animal fibre can be best distinguished. Thus the inelastic fibres swell, coalesce, and disappear, when acted upon by acetic acid; the muscular or contractile become yellow with nitric acid, and, upon the addition of water of ammonia, change to orange; and the elastic fibres obey neither of these reactions. Neither the muscular nor elastic fibres dissolve by the most prolonged ebullition in water; the inelastic dissolve when boiled in a Papin's digester, yielding gelatine; and the homogeneous structureless basis of cartilage also dissolves under similar conditions, but the solution is not precipitable by tannic acid, which is the case with gelatine.

The nerves are tubes of inelastic fibre (neurolemma), filled with an oleo-albuminous emulsion. After death this emulsion separates, the albumen fixes itself to the internal lining of the tube, and the oil collects into drops. The grey or sympathetic nerves also contain fibres (bands of Remac) in appearance and chemical characters extremely similar to the fibres of the involuntary muscles.

The arterial parietes contain a mixture of the inelastic, the

elastic, and the contractile fibres described. Similar in structure are the walls of the uterus. The veins have inelastic and elastic fibres. The basement membrane of glands is, for the most part, composed of inelastic fibres.

We cannot now enter into the description of the peculiar bodies found within the nervous centres; the remarkable organs connected with certain nerves, the distribution of the various anatomical elements in the structure of the conglomerate and other glands, the variety of cellular formations on the internal surfaces of their ducts, and the important relations of these formations with the process of secretion, are subjects likewise too vast in extent, and too great in importance, to be cramped into a notice such as the present. We merely wish to draw the attention of our readers to the department of microscopical anatomy, and to assure, by a few examples, that the study is one replete with interest, as it is pregnant with most valuable results. Above all, we wish to impress the importance of this study on those who are anxious to cultivate pathology. Nothing is better ascertained than that tumours, in whatever situation they may be placed, are principally composed of the histological elements of the adjoining parts. How useless is it then to waste our time in examining diseased structure, unless we are familiar with the microscopic appearances of the natural constituents of the human body? The argument has but to be stated to be admitted, and yet how many have, without hesitation, given their opinion as to the malignancy or non-malignancy of a suspicious tumour, and that professedly from the assistance of the microscope, who could not distinguish between a section of a ligament and of an involuntary muscle, if exhibited to them in the focus of that instrument.

We cannot conclude without rendering to Mr. Hassall and the publishers of the "Microscopic Anatomy of the Human Body," our sincere thanks for the benefit they have conferred upon the science of our profession.

A Practical Treatise on the Domestic Management and most important Diseases of advanced Life, with an Appendix, containing a series of Cases, illustrative of a new and successful Mode of treating Lumbago and other Forms of Chronic Rheumatism, Sciatica, and other Neuralgic Affections, and certain Forms of Paralysis. By George E. Day, M. D., &c. London, T. and W. Boone. 1849. pp. 342.

The English language, so rich in valuable treatises on all other branches of medicine, is singularly deficient in works devoted to the consideration of maladies which accompany the decline of life; and the effects produced by advanced age on the ordinary diseases to which man is liable are but cursorily noticed by writers on pathology and therapeutics. The late Sir Anthony Carlisle, in a work more popular than medical, published about thirty years since, described some of the disorders of old age, and proposed means for prolonging life. He was followed by Sir Henry Halford, who, in his truly classical essays and orations, without writing a set treatise, put forth some most valuable dogmas on climacteric disease. We next find the subject ably handled by Dr. Holland, in his Medical Notes and Reflections, whose writings, and those of Sir Henry Halford, we doubt not most of our readers have perused with benefit. We cannot call to mind any other work in our language in which these diseases are specially noticed, except that of Dr. Marshall Hall on some of the more important diseases of females, and the essays in our Cyclopædia, Dictionary, and Library of Medicine.

Dr. Day has therefore done good service to the physician in applying the stores of his well-cultivated mind to a further elucidation of the difficulties so often met with in managing the affections of the aged, and pointing out how they differ, both as regards their diagnosis and treatment, from those of younger life. The task he has undertaken he has executed well, and we confidently predict that the pen of so ready a writer, when matured by experience, will do much to advance practical medicine.

In reviewing this excellent volume, we purpose to present our readers with a short sketch of its contents; to do more would be supererogation, as it is one that must find a place in their libraries, to be often referred to in daily practice.

The first three chapters may be in some sort deemed introductory; they are devoted to the consideration of the changes which occur in the system, the preservation of the health,

and general observations on the medical treatment, of advanced life. It is of the utmost importance to be well acquainted with the alterations in the position of the internal organs which advancing age produces, and also with the changes which they undergo both in structure and function. Dr. Day is well aware of this, and notices these points at some length, as they affect, 1st, the respiratory system; 2nd, the nervous system; 3rd, the digestive organs; and, 4th, the organs of circulation. The modification in the respiratory organs and their functions he describes as follows:—

"The osseous case enclosing the lungs and heart is usually altered in form. Its upper portion is flattened at the sides, causing a well-marked diminution of the transverse diameter. The vertical diameter is shortened in consequence of the diminished height of the intervertebral cartilages, and as the anterior portion of the cartilages is most rapidly absorbed, we have forward curvature of the spinal column, as a further cause tending, in conjunction with the others, to lessen the capacity of the chest. The intercostal spaces lessen in size, till contiguous ribs come in contact with one another, and the cases are not rare in which, in extreme age, they are found actually united.

"The ribs diminish in density, and lose their elasticity, and the cartilages of the two first are usually ossified. Union of the chondrosternal articulations is not very common, and the joints uniting the ribs with the spine generally preserve their mobility. The inferior boundary of the thoracic cavity,—the diaphragm,—is naturally altered in form, in consequence of the preceding changes. We find it thrown in folds, the impressions of which are often well marked on the upper surface of the liver.

"The lungs of aged persons vary considerably in their aspect. These varieties have been comprehended by Hourmann and Dechambre in three typical forms, by which one lung may differ from another, or parts of the same lung from other parts. I have slightly modified their views in a few points.

"*First Type.*—In muscular persons, with well-formed thorax, not very much altered by the ravages of age, the lungs present little apparent difference from those of the adult, except in the altered position of the great interlobular fissure. The degree of this alteration corresponds with the extent of lateral flattening. In the adult this fissure lies immediately beneath the upper lobe, and passes obliquely to the root of the lungs, so that on the right side the central lobe occupies exactly the middle part, and on the left side has the lower lobe immediately beneath it. But in old age the fissure approximates to the vertical direction, so that one lobe of the left lung is directly in front, and the other behind, and the middle lobe of the right lung projects downwards, and the lower lobe becomes elevated behind it, so as to form the posterior fourth (or more) of the summit of the organ. On examining a thin dried section under the

microscope, the cells are found to be about double the size observed in a similar section of an adult lung.

"*Second Type.*—The lungs are of regular form, but small, light, and hardly capable of being distended sufficiently to fit the thorax, even by the strongest inflation, and when thus inflated, the air escapes more readily than from the adult lung. On pressing them between the fingers, the ordinary crepitation is entirely, or for the most part, absent. They are bathed in a clear serous fluid. The cells, on examining a dried section, are found to be larger than in the preceding type, and there is an obvious diminution of the fine vascular tissue that is so apparent in the adult lung.

"*Third Type.*—The lungs lie closely attached to the vertebral column, and form an irregular mass surrounded by much serous fluid. They are livid and flabby, and their normal conical form is no longer recognizable, the summit being often larger than the base. The fissures have sometimes apparently disappeared, and the lobes are merely united by a flat, thin pedicle. Inflation does not much increase their size. They are very light, and communicate to the touch the sensation of a skein of flax. In this case the microscope exhibits cells of a highly irregular form, but of larger dimensions than in the preceding types. Very few vessels are perceptible.

"These facts verify the law announced by Magendie, that the density of the lungs diminishes, as also does the quantity of blood they admit, with the progress of age. The thorax gradually accommodates itself to this change; it becomes atrophied as the lungs atrophy; it contracts as they contract. The effusion of serum may be owing, at least in part, to the chest being unable to contract beyond a certain point, so that, as the lungs diminish, the serum may fill the vacant space.

"There is yet another point to be noticed in connexion with the lungs of aged persons, namely, the black deposit which is frequently found in the respiratory organs. It seems due to the stagnation of the blood in the pulmonary tissues. When it occurs in great abundance it is capable of producing considerable inconvenience by obstructing the capillary vessels and respiratory canals. The absorbents diminish with the advance of old age. The fibrous tissue of the bronchial tubes not unfrequently becomes hypertrophied, and thus lessens their capacity; and their contraction is often much increased by chalky deposits in the sub-mucous tissue, by thickening of the mucous membrane from repeated inflammations, and from various other causes. Ossification of the thyroid and crycoid cartilages, and of the rings of the trachea and bronchi, presents an additional obstacle to the free entrance of air.

"Let us now consider the modifications in the functions of these organs consequent on the above changes.

"As the respiratory organs become worn out, the necessity for employing them seems to diminish in a corresponding degree. The movements of the chest, especially in the transverse direction, are much diminished, and become irregular both in number and in

duration. The diaphragm becomes the chief agent in inspiration. The scaleni and sternocleido-mastoidei muscles are rendered almost useless when the spine is much curved; and to remedy this, the head is thrown back at each inspiration, so as to put these muscles to the stretch. Ordinary expiration is sudden and rapid; forced expiration, as in coughing and expectorating, is difficult when there is much curvature, in consequence of the relaxed condition of the recti, obliqui, and transversales muscles. From the examination of 255 women, ranging in age from sixty to ninety-six years, Messrs. Hourmann and Dechambre found that the number of respirations in a minute averaged twenty-two.

"The diminution in the expansive power of the lungs and in the extent of exposed mucous membrane, and the decrease of the vascular tissue, sufficiently indicate that the changes produced in the blood while passing through the lungs must be very different from those occurring in the adult. The change from venous to arterial blood cannot be efficiently brought about. The circulating fluid leaves the lungs with the impress of venosity still adhering to it. The carbon, which ought to be eliminated by the lungs as carbonic acid, is only imperfectly removed, and the corresponding volume of oxygen which ought to be absorbed in its place, and conveyed in the blood to every cell within the organism, carrying with it health and vigour, can no longer gain admittance into the system.

"Moreover, the altered state of the vascular tissue and of the bronchial mucous membrane presents an obstacle to the free escape of the aqueous vapour charged with organic matter, which is usually evolved from the pulmonary organs. In consequence of this retention the lungs have a tendency to become œdematous, and the depositions so frequently found in the lungs of the aged are probably, in some measure, dependent on the same cause. That the character of the pulmonary excreta also varies is obvious from the fetid odour of the breath, not unfrequently noticed. If the urinary secretion is checked, the breath often has an ammoniacal smell."—pp. 6–11.

In connexion with the functions of the nervous system, Dr. Day notices a point often overlooked by the physician in his diagnosis as to the amount of disease present in any organ, namely, *the insulation of the different organs* which exists in old age. If, for example, we judge of the extent to which a lung may be diseased by the condition of the heart's action, so important a diagnostic aid in youth and adult life, we shall be often very much deceived, for in old age the great systems of life seem to act to a great extent independently of each other, or, as the author terms it, are *insulated:* thus one of the lungs may be so extensively diseased as to be almost perfectly useless for the discharge of the function of respiration, and yet the heart give little indication of the obstruction necessarily

thrown thereby on the circulation of the blood. The increased difficulty in diagnosing accurately the diseases of advanced life, caused by this peculiarity of the nervous system, is apparent, and, taught by it, the physician should be especially cautious in his prognosis, in all affections of the aged, when a vital organ is engaged. The young practitioner, unaware of this fact, on which so much stress is correctly laid in Dr. Day's volume, is often surprised at the suddenness of death in old persons affected with what the ordinary symptoms lead him to believe a trifling affection, and still more so with the extent of disease which a *post mortem* examination reveals in such cases, and which during life he did not detect. The author quotes the following apposite remark of Prus on this point: "Enter a ward devoted to the treatment of the diseases of old age, and you will be struck with the thorough indifference with which a patient sees his neighbours die around him: so it is with the economy of old age; the body is destroyed piece-meal without any general reaction, without the appearance of conservative effort."

The want of agreement between the pulse felt at the wrist and the action of the heart, is judiciously pointed out, and the useful rule laid down, that, in the diseases of old people, "*the pulse should be counted at the heart.*" Dr. Day here notices a mistake made by most physiologists in regarding it as an established fact, that the number of the heart's pulsations diminishes in advanced life. From the data afforded by 562 healthy women, whose mean age was seventy-three years, he ascertained that the average number of pulsations in them was a fraction above 79 in a minute; and that the average pulse of 197 healthy men, of the mean age of sixty-eight years, was 72·5. He concludes from this, that individuals with very slow pulse in old age are exceptional cases. Our own experience furnishes us with many instances of quick pulse in old persons; thus, in one lady aged nearly 70, at present under our care, the pulse is seldom below 80 in the minute in her best health, although she exhibits no evidence of diseased heart; but, were we to speak in general terms, we should say a slow pulse is more common than a quick one in advanced life. There is another point connected with the pulse in the aged, which Dr. Day has omitted to mention, and which is of much practical importance, namely, that change of posture has much less effect on the pulse in advanced life than in youth or adult age; such, at least, is our experience, and we think it would be well to test its truth experimentally on a large scale.

The precepts for the preservation of health in declining life,

as given in the second chapter, are most valuable. Every point is ably treated, and the most trifling not neglected; the habits, time for meals, choice of food and drinks, clothing, all receive a due amount of consideration; and rules of the utmost importance for the aged who wish to preserve a *mens sana in corpore sano* laid down. The sleeplessness from which old persons occasionally suffer so much is fully noticed, and the author's experience of chloroform in its treatment, which he had not sufficiently tried when the body of the work was going through the press, is given in an introductory note. He speaks most highly of its effects, having arrived at the conclusion that "there is no age at which chloroform may not be given with perfect safety with the view of procuring sleep." Dr. Day lays especial stress on the necessity of guarding against atmospheric vicissitudes, in old age, by warm clothing; and seems to think that there is much truth in the opinion of the ancients as to the possibility of the aged prolonging life by sleeping with young persons:

"Old persons commonly suffer very much from the cold during the night. They should be thoroughly warm on going to bed, and in cold weather should sleep with their feet resting upon a stone or tin bottle filled with hot water, and thickly enveloped in flannel. There is, I believe, no measure so influential in supporting the sinking vital energies of age as the communication of animal heat, particularly from the young of our own species. This is one of the oldest(*a*) restorative means of treatment practised, having been adopted by King David when he 'was old and stricken in years; and they covered him with clothes, but he gat no heat.' Boerhaave caused an old burgomaster at Amsterdam to sleep between two young persons, and we are informed that the old man acquired, by this means, a visible increase of vigour and activity."—pp. 35–36.

The third chapter is fraught with instruction to the unskilled in the ailments of the aged, as to the general principles

(*a*) "The following inscription discovered at Rome, in the seventeenth century, seems to allude to this subject.

Æsculapio et Sanita
L. Clodius Hermippus,
Qui vixit annos cxv. dies v.
Puellarum anhelitu.
Quod etiam post mortem ejus
Non parum mirantur physici
Jam posteri, sic vitam ducite.

This inscription gave rise to a very singular work by Dr. Cohausen, entitled, "*Der wiederlebende Hermippus, oder physikal. u. med. Abhand. von der seltsamen Art sein Leben durch das Anhauchen junger Mädchen auf* 115 *Jahre zu verlängern.* 1753."

in their treatment; the difference of sex in relation to disease, and the want of reaction in all morbid affections of advanced life, are adverted to. The effects of loss of blood on the old are specially noticed, and due caution recommended in the use of either general or local bleeding; we fully agree with Dr. Day that "it is a measure that must be adopted with extreme caution and comparative limitation." We feel happy, however, to think that the present school of physicians, warned by the ill consequences which ensued from the promulgation of Broussais' doctrines, so freely adopted for some time, require not to be told of the dangerous results of indiscriminate bloodletting at any period of life, least of all when nature's decay requires the *pabulum vitæ* not to be diminished.

Climacteric disease is treated of in the fourth chapter. The term climacteric, originally applied by the Greek physicians to designate the five epochs of life, is now applied solely to the changes which occur about the sixty-third and eighty-first years, and which were called by them the grand climacterics. These may be, as mentioned by Dr. Day, either of renovation or decay. There is one example adduced by the author as a proof of the former, namely, those cases in which old persons recover their powers of vision so far as to be able to lay aside the use of spectacles; but this fact, although frequently quoted by writers as an evidence of renovation of constitution, —we have heard it lately mentioned as such with respect to the Duke of Wellington,—is but one of the effects of old age; the projecting cornea, on which the near-sightedness of old age so generally depends, becomes flattened by the absorption gradually going forward, which accompanies the farther advance of life.

In the fifth chapter is shortly described senile marasmus or wasting, its symptoms and treatment; and in the sixth, the author's deductions from the tables of the Registrar-General of England, on the diseases most fatal to persons in advanced life, are given. The conclusions arrived at are, that of 1000 persons dying at and after 60, 285·3 die of old age, 237·5 of diseases of the respiratory organs, 130·9 of diseases of the nervous system, 59·2 of diseases of the digestive system, 53·5 of diseases of the circulatory system, 20·3 of diarrhœa, 14·2 of influenza, 7·8 of erysipelas, and 191.3 of other diseases, as typhus, dropsy, diseases of the urinary organs, cholera, dysentery, cancer, gout, rheumatism, &c.

Diseases of the respiratory organs, then, as might *a priori* be supposed in the changeable climate of England, are more than doubly as fatal to old persons than those of any other of

the great systems; and from the detailed tables of Dr. Day, it appears that exactly one-third of those dying from affections of the lungs are carried off by bronchitis. We, however, agree with him in thinking that pneumonia is present in a great proportion of those said to die of bronchitis; and it must be remembered that the tables of the Registrar-General, so far as regards the exact cause of death, can only be looked upon as approximations to the truth.

The remaining chapters of Dr. Day's volume are devoted to the special consideration of the most common diseases from which old persons suffer, and to an elucidation of the specialties in their diagnosis and treatment as contrasted with similar affections occurring in earlier life. Diseases of the lungs, as shown to be the most important, are first considered, and practically illustrated in a series of instructive essays, forming the various sections of the seventh chapter. Influenza is described chiefly from the author's experience of the late epidemic, fully noticed in our review of Dr. Peacock's work, in a late Number(*a*). The author's remarks on its treatment are, we think, too concise, and he does not seem to be aware of the practice we so strongly recommended in that review, of administering at the very commencement a strong opiate, with the view of cutting short the disease; we have seen it effective in even the very aged.

Diseases of the nervous system affecting the aged are next concisely noticed, there being but little peculiarity in them as regards their occurrence in advanced life. The ninth chapter presents an admirable *resumé* of the ailments of the digestive organs from which old persons so uniformly suffer, especially functional derangements of the stomach and intestinal canal. We cannot forbear quoting from the third section of this chapter the following excellent remarks on senile jaundice:

"There is a peculiar form of jaundice, to which old persons are subject, and which has consequently received the name of *icterus senilis*, or *senile jaundice*. It is preceded for some weeks, or even for months, by a feeling of pressure and general uneasiness in the pit of the stomach, often amounting to actual pain, and most perceptible a few (two to four) hours after taking food. In some cases shooting pains are felt, extending from the stomach to the right hypochondrium, during the whole process of digestion. The skin in the mean time presents nothing more than a cachectic appearance, which is however gradually replaced by a true jaundiced tint.

(*a*) Vol. vii. p. 433.

The above symptoms at first intermit, leaving the patient in a state of comparative health for some weeks; the intermissions, however, gradually become shorter, and at last altogether disappear. The skin finally presents a dull blackish green tint. The urine is dark from the presence of bile-pigment, and deposits a copious sediment. The state of the intestinal excretions varies considerably; there may be extreme constipation, or there may be greyish clay-like motions, or finally these last may be intermixed with black, pitch-like bile. The pulse is very slow. The patient complains of a dull aching pain under the false ribs of the right side, which is increased by a deep inspiration. A careful manual examination will frequently detect a hardness or tumour, the exact seat or nature of which it is often difficult to ascertain. It may be situated in the liver, the pancreas, the spleen, or the omentum. There is a gradual loss of appetite, accompanied with nausea, and often with vomiting; the patient wastes away, hectic fever is set up, the feet and abdomen become dropsical, and the emaciation finally becomes extreme. During the last stage of the disease, we frequently find that nothing can be retained on the stomach, and the peculiar matter resembling coffee-grounds is vomited, which we may regard as affording pretty certain evidence that there is organic disease of the liver, gall-bladder, pancreas, or stomach. Death ensues from exhaustion, with convulsions and delirium, or from coma.

"It will be seen that I have thus classed, in a single category, all forms of jaundice depending on structural changes. The symptoms in all cases are pretty much the same, and I fear we must add that, generally speaking, the treatment is alike unavailing. While gall-stones are most common in the female sex, men are most frequently the subject of the present disease. It is most common in old spirit-drinkers, in hypochondriacs, and in those who have been liable to attacks of gout or of hæmorrhoids; in fact, it seems closely connected with a plethoric and torpid condition of the portal circulation. A sedentary mode of living, the use of unwholesome and indigestible food, constipation, the suppression of natural or artificial discharges, and, above all, the perpetual struggle for mere existence, to which thousands in this metropolis are subjected, are amongst the principal causes favouring this disease.

"The course of this disease, or, more correctly speaking, of the diseases on which this symptom depends, is very slow. The patient may live for years, the pain in the hepatic region usually increasing. A perfect restoration to health is only possible where the jaundice depends on the impaction of a concretion, or on the pressure dependent on an accumulation of hardened fæces. Most of these cases terminate in dropsy and hectic fever, a few in apoplexy, and fewer still in intestinal hæmorrhage.

"The treatment is seldom more than palliative, for the structural changes giving rise to the jaundice are usually beyond the resources of our art. Much may be done, in some cases, by a judicious regulation of the diet, and the regular administration of mild

laxatives and anodynes. Diet-drinks, containing taraxacum, burdock, or sarsaparilla, combined with muriate of ammonia, liquor potassæ, or small doses of bichloride of mercury, have checked the progress of the disease for a time. The free inunction of iodide of lead ointment may be associated with the above internal remedies." —pp. 217–221.

Chapter X. contains a description of the diseases of the heart, and Chapter XI. of diseases of the urinary organs, both most important in certain peculiarities, as they affect the aged. Dr. Day's observations on them are highly practical, and deserve to be carefully perused.

Diseases of the skin, ulcers of the legs, senile gangrene, and gout and rheumatism, form the subjects of the twelfth and thirteenth chapters; and the volume concludes with an appendix, in which is given an account of the thermic treatment of lumbago and other forms of chronic muscular rheumatism, sciatica, and other forms of neuralgia, paralysis, &c., with the author's experience of its efficacy in these various affections when they occur in old persons.

On Gout, its History, its Causes, and its Cure. By WILLIAM GAIRDNER, M. D. London. 1849. pp. 232.

THE author informs us in the preface, that, in writing the history of gout, he has, in the work under review, confined himself to the pure type of the disease, in which its real nature is best seen; it is therefore intentionally that no reference is made to its complications with rheumatism, hypochondriasis, and insanity, these subjects, although highly interesting, being so very extensive as, if introduced, to swell the work far beyond the limits within which he desired to confine the present one.

When alluding to the obscurity and great frequency of gout, Dr. Gairdner explains his reasons for publishing the present volume; he says:

"Not any of the diseases to which man is liable is a cause of greater perplexity and disappointment than gout; yet this does not arise from the oft-repeated reproach of its intractable nature; it may indeed be said with truth that it is more curable than many, and that it is certainly more amenable to relief than most diseases which fall under the cognizance of physicians. The regular attack of gout cannot be said to give more embarrassment to a medical attendant than the assaults of any other malady, and inasmuch as they are, for the most part, quite exempt from danger, they give little solicitude for the event; but it is quite otherwise with its irregular

forms. In the beginning of a physician's practice, especially while he is as yet unfamiliar with any but the noted and typical forms of disease, the changing and mysterious phenomena of gout, and particularly its complications with other disturbances of the system or injuries of parts, are full of doubt and difficulty. In the earliest years of my professional life, my mind was frequently called to its consideration, to the observation of its various forms, the unravelling of its strange and confusing associations with other known forms of disease, and meditation on its cause and nature. I well remember how often I was perplexed by its obscure indications, how often I was surprised to discover it lurking unsuspected in the system, disturbing the healthy functions, and how much I was puzzled by seeing the symptoms of other diseases swayed from their natural and ordinary course in consequence of the intermixture of gout."

"I have hailed with great interest the works which have occasionally proceeded from some of our ablest physicians during my time, and particularly some writings which have within these few years issued from the press; but though I have found much matter of instruction in them, I have been mortified not to discover certain observations and thoughts of my own reflected in the minds of my colleagues. Recollecting that my own opinions had undergone great changes, I have been led to further inquiry, and although on certain points I may feel doubts strengthened, there are others on which re-examination and reflection only bring additional conviction. What these are will sufficiently appear in the following pages. Whether my interpretations are true or not, my contemporaries must judge; but believing that it may be of use to promulgate opinions earnestly and honestly formed, not so much from reading as from observation and meditation, I venture, with diffidence indeed, to add another volume to the many already written on the subject."

The book is divided into eleven chapters, the three first being occupied by a consideration of the frequency, history, varieties, sequelæ, and terminations of gout. Dr. Gairdner gives a very faithful description of the regular and atonic gout, the former of which, for convenience sake, he divides into three stages, marking its nature by the effect on the constitution, and guiding our practice by an accurate appreciation of the inroad it has made on the constitution.

The fourth chapter treats of the different theories of gout, morbific matter, indigestion, and uric acid diathesis. With regard to the term morbific matter, our author says that—

"In describing this disease we are almost irresistibly led to employ a figurative and somewhat poetical language; its phenomena readily adopt themselves to the metaphor of a surging mass, a coction, or a wandering vapour."

But he conceives it a violation of all the best rules of philosophy to explain obscure phenomena by means of an assumed principle, or fact of, at least, doubtful existence; and although several physicians of eminence of our own day have openly expressed their belief in a *materies morbi*, yet he says it is undoubtedly true that this easy and profitless explanation finds little countenance from physicians in general:

"Another opinion, however, prevails, which, though it leads to a sounder mode of treatment, seems to me to involve hardly less of error. The very frequent accompaniment of symptoms of indigestion has led to this theory by a natural and strong impulse. But if it be considered how often the disease exists in its several forms, with little, if any, sign of dyspepsia, and that in its first invasion, when the real nature of the complaint may be supposed to be most clearly indicated, and its accidental complications to be least manifest, the signs of depraved digestion are nearly always absent, it must be admitted that some other cause should be sought for. It is, indeed, proverbial that the first attack of gout is attributed to a sprain, because it supervenes in the midst of the rudest and most perfect health."

A very experienced physician once told the author that he was generally "taken in by the first attacks of gout, not being able to make up his mind to pronounce in favour of so formidable a disease on the evidence of one symptom in the midst of such perfect health." On the other hand, he has met physicians who had gout so constantly before their mind, that they were ready to suspect the presence of the disease in all circumstances, and who, in fact, were like the celebrated surgeon who discovered a syphilitic taint in every obscure case brought to him, and who would not admit, in favour of his patient, the fact that he had not been exposed to infection: "If it was not you, it was your father, and, if he was not guilty, your grandfather was."

The author conceives that all that has been proved with regard to indigestion amounts to this: that it is the most constantly attending symptom of gout, and that from a consequence and satellite it may easily be erected into a cause. When considering the subject of the uric acid diathesis, Dr. Gairdner says:

"One of the most remarkable results of arthritic indigestion is the presence of acid in nearly all of the excretions of the body; this has of late years been called the *uric acid diathesis*. The adoption of such a term would imply a conclusion, to which I am by no means prepared to assent. By a diathesis I understand a condition of the constitution, and not a simple affection of certain fluids of the body.

Dr. Garrod has lately much extended our knowledge by a very interesting proof of the existence of uric acid in the blood."

And the facts of Wollaston having previously found out the existence of uric acid in the joints, Landerer in a concretion between the coats of the aorta, and its being discovered in several of the fluids of the body in a state of disease, does not, our author thinks, justify us in the use of the expression; and he is of opinion that the uric acid, as a cause of gout, meets with a fatal difficulty from the fact, that urates in the urine are common to gout with many other diseases: ordinary indigestion, from whatever cause, or a slight and ephemeral fever, will cause this appearance. After alluding to the valuable researches of Dr. Garrod with respect to the presence of uric acid in the blood, our author proceeds to the fifth chapter, in which he treats of the formation and relations of urea and uric acid; and the conclusion that he arrives at with respect to the relations of these substances, after a series of analytical examinations of the urine in gouty patients, which are given in detail, is:

"That both urea and uric acid are formed from the blood during the processes of assimilation and oxygenation. When this latter process is perfect the uric acid nearly disappears, but if circumstances occur which interfere with the due arterialization of the blood, the immediate consequence is the appearance of uric acid in the urine. It is probable that the elements of the urea and uric acid are partly derived from the disintegration of the tissues; but it would be difficult to account for the great varieties which occur in man in health and disease, and in all animals in the normal state, without supposing that the food much influences their urinary secretions. I have no doubt that many physicians will think these ideas are pursued too far in a work on practical medicine, and that chemistry can hardly yet be called on to solve questions so intricate. They cannot require more prudence in the application of chemistry to the uses of medical science than I am disposed to incultate, and I should place little reliance on these results did they not meet with confirmation from the other facts of this disease, but when we meet with an occurrence in the chemical history and pathology of gout, and the same explanation of what would be otherwise doubtful, I am encouraged to adopt them without hesitation. I trust that I have convinced my readers, as well as satisfied myself, that in gout we have not merely an increase of urates in the excretions, but an altered relation of the urea and uric acid, and that this change takes place in the early assimilation of the food, during the process of respiration. These facts throw much light on the pathology of the disease, and they are so distinctly applicable to treatment that I trust even those physicians who deprecate the great freedom with which speculations of chemistry have sometimes been used to bolster up fancies in physic,

will excuse the length and minuteness of the details into which I have entered."

We must here introduce the following passage from the introduction to the work:

"The opinions I have expressed concerning urinary deposits are at variance with some which prevail, but I have been at much pains to be accurate, and I have reflected carefully before venturing to claim public attention to them; the subject is speculative, although resting on observation. The facts related should be confirmed by others, and the experiments repeated by other hands. I trust the whole subject will pass through this ordeal; if I am in error I shall freely and candidly withdraw what I have advanced; if my opinion meet with the assent of my profession I shall be glad to have added to our knowledge of a difficult subject."

Dr. Gairdner does not consider inflammation essential to gout, but refers its origin to plethora from visceral obstruction, on which subjects he explains his views at length in the seventh chapter.

The ninth chapter is devoted to the curability of gout, together with rules as to diet, exercise, &c., to be pursued in infancy and youth, with the view of counteracting the hereditary disposition to gout, and is replete with judicious directions on these all-important subjects.

In the concluding chapters our author enters at length into the treatment of the regular and irregular forms of gout. With respect to blood-letting in regular gout, Dr. Gairdner is, in common with other judicious physicians, much opposed to even ordinary bleedings, unless there be inflammation of some important internal organ, but he says that,

"Rejecting altogether the opinion that gout is in any degree of an inflammatory nature, and believing that such a blood-letting as is necessary to subdue any of the phlegmasiæ is most injurious, it does not follow that this remedy may not be employed in such measure and with such precautions as will surely avoid the above great and recognised evils, and yet give that relief to an overloaded heart and vessels which the symptoms of the disease point out as required."

Our author further on states his conviction that the use of small bleedings is hardly understood by us, and that the late Dr. Parry, of Bath, as far as he knows, was the only physician who made use of them. Our author conceives that small bleedings, instead of debilitating, may be ranked as tonics, on account of the cheerfulness, energy, and sense of power they suddenly restore to persons oppressed by a state of plethora; and they,

according to him, supplant at the same time the necessity of purgatives, which are very often noxious in gout, and attended with considerable risk. He says that it may seem absurd to open a vein in order to abstract from three to six ounces of blood, but affirms that they will not say so who will make a trial of it.

With regard to small bleedings in cases of regular gout, we are of opinion that at this side of the Channel at least there are few cases uncomplicated with inflammation of some internal organ, in which we would consider ourselves justified in having recourse to general blood-letting, even to the small extent recommended by Dr. Gairdner, knowing from experience that the paroxysm can be brought to a happy termination, in general, by milder modes of treatment; and we therefore cannot agree with Dr. Gairdner that it is unfortunate that the prejudices of our patients very often oppose themselves to its use.

Dr. Gairdner says:

"I have repeatedly used this remedy with the happiest consequences, in the case of men who were affected with symptoms of incipient gout in the midst of the heaviest occupations of life. I was once sent for to see a gentleman who, at the time of his seizure, had given notice to bring forward great public business in Parliament. He had been bled before in similar circumstances, but had suffered so much afterwards that he was very averse to return to the use of the same remedy; but he was a person of very dispassionate judgment, and not wholly unacquainted with medicine. On hearing my explanation, he consented to lose four ounces of blood. This small quantity was quite sufficient to relieve the system; he made his speech with great facility, and told me two years afterwards that he had not for ten years previously enjoyed such uninterrupted good health. I ought, however, to add that he was a person who could place restraint upon his desires."

Dr. Gairdner, however, never makes use of this remedy in cases of gout in an impaired or defective constitution, to which he considers it wholly inapplicable, believing, as he does, that some considerable degree of vigour is necessary for its legitimate use, which he conceives is generally met with in the early stages of regular gout.

With regard to the use of colchicum in regular gout, Dr. Gairdner is of opinion that its character as a specific is quite established, and we look on his observations as to the mode of administering it as of great importance. He considers that the most common reason for its failure is the unnecessarily large dose in which it is frequently ordered. He recommends it in doses varying from ten to fifteen minims, and that not until the first severe

symptoms of the disease had been subdued; and he says that he finds, if there be sufficient vigour of constitution to permit the practice of a small bleeding, that colchicum may be used much sooner, and its administration attended with happier results. Our author does not coincide with Dr. Christison, who declares that he never saw the benefit of colchicum conferred till it had produced griping, purging, or some disturbance of the *primæ viæ*. According to Dr. Gairdner's opinion, colchicum never more effectually relieves the patient than when it acts silently and peacefully, without producing any evacuation whatever, or in any way disturbing the patient's comfort and ease. Its *modus operandi* he, with others, considers anything but involved in mystery, Dr. Douglas Maclagan, Professor Chelius, and Dr. Lewins, having demonstrated that it causes a more copious discharge of urea from the system; and he has found, by repeated investigations, that the increase of urea was attended by a great diminution of the urates in the urine: and he states that the opinion he has expressed in another part of the work receives confirmation here, viz., that urea and uric acid are again found to be correlative and vicarious substances. Further on, Dr. Gairdner says:

"When I consider how much gout is influenced by the condition of the nervous system, and that the most notable effect of colchicum, whether acting as a remedy or as a poison, is that of a narcotic, and when I further consider the great rapidity of its action, it appears to me probable that the nervous system will be found to be the principal channel through which this medicine exercises its powers."

This chapter, together with the concluding one on the treatment of irregular or metastatic gout, contains a great deal of useful practical information. Dr. Gairdner has evidently devoted considerable attention to the subject of gout, and has taken advantage of an extensive opportunity of becoming thoroughly acquainted with the disease. It is on this account that we have quoted him at such length, and we cordially recommend a perusal of the book itself.

A Remonstrance with the Lord Chief Baron, touching the Case Nottidge v. *Ripley.* By John Conolly, M. D., Physician to the Middlesex Lunatic Asylum at Hanwell. 8vo. Second Edition, pp. 19. London, Churchill. 1849.

A Letter to the Right Hon. Lord Ashley, relative to the Case Nottidge v. *Ripley.* By T. T. Wingett, M. D., Medical Superintendent of the Dundee Royal Lunatic Asylum. 8vo. pp. 16. Dundee, Chalmers. 1849.

The Journal of Psychological Medicine and Mental Pathology. Edited by Forbes Winslow, M. D. No. VIII. October, 1849. London, Churchill.

Few cases have ever come before the medico-legal world of such interest as the recent one of Nottidge *v.* Ripley. Generally the interest taken by the public in lunacy questions is derived from the wealth or importance of the lunatic; in this case it is altogether dependent upon the momentous questions which have arisen during the course and since the termination of the trial, as to what shall be the test for the future of the propriety of confinement in a given case of madness. The debate, we repeat, is not whether such and such constitutes insanity, but whether such and such constitutes *sufficient* insanity to render confinement necessary.

The facts of this case are simply these:—Miss Nottidge, the plaintiff, is the eldest daughter of a widow lady of fortune, residing in the country. Three other daughters, each having £6000 at her absolute control, have been, under the influence "of their religious feelings, and perhaps a natural tenderness of nature," subjected to the influence of a certain curate named Prince, one of the English clergy, who is accused of having deluded this imbecile maiden lady. This person has three followers of his own sex, who speedily become, as Dr. Conolly says, "the extemporaneous husbands" of these three ladies. They betake themselves to a place in Somersetshire, called the Agapemone, or Abode of Love, whereof Mr. Prince is the head. The three husbands immediately obtain possession of the £18,000, or, in other words, of the sole property of their deluded wives. Miss Nottidge accompanies her sisters to the Agapemone, but, after a short time, returns home disgusted. Believing it to be the will of God, that is, of the god of the Agapemone, she returns, her place of residence not being communicated to her mother.

It is now determined that she shall be put under the protection of a lunatic asylum; she all the time, "with a faith surpassing the faith of her sisters, declaring that Mr. Prince is the Almighty." She is placed under the charge of Dr. Stilwell, of the Moorcroft House Asylum, at Hillingdon; but at the end of seventeen months she is liberated by order of the Commissioners, owing to the impairment of her health from confinement. We must add, that her original delusions still existed in full force, and that the Commissioners, including the late lamented Dr. Prichard, were unanimous in the opinion that she was *insane*, and a proper object for confinement in a lunatic asylum. The result of the liberation was the action against Ripley, her brother-in-law, to recover compensation in damages for incarceration in the asylum. A verdict was obtained in her favour, the Lord Chief Baron directing the jury that if the evidence did not prove that the plaintiff "was dangerous to herself or to others," she was entitled to damages. In his charge, the Judge made many observations involving important considerations, which want of space, and the fact of their being already ably commented upon in the recent letter of the Commissioners in Lunacy to the Lord Chancellor, prevent us more fully entering upon. But there is one remark made by the learned Judge, which, though interwoven amid a tissue of error, peculiarly calls for animadversion. He is reported to have stated that his own idea of toleration was, that all those who entertained with sincerity any peculiar doctrine, however absurd that doctrine might appear to others to be, ought to be allowed to enjoy that opinion without interference, so long as the principles and the acts they adopted were not forced offensively, or contrary to law, upon the public notice, or against the public morals. Well does Dr. Conolly observe, with obvious reference to this part of the charge to the jury, that no suspicion seems ever to have flitted across the bench that this Abode-of-love god, who signs himself Amen, and has destroyed prayer for ever, and hastened the coming of the kingdom of heaven, and placed the game of hockey among the immortal recreations of ladies and gentlemen, ever extended his power beyond the securing of their property, or that there may be places where a mock religion and a boundless fanaticism sanction modes of worship which tend to destroy all sense of modesty. But a still weightier objection lies to the remark of the Lord Chief Baron. A distinction must, for the obvious reasons, be made between an abstract belief in a doctrine, such, for example, as that the Day of Judgment is past, and the belief in a certain man being God, for such a belief as the

last is incompatible with the due performance of the social relations, and as the influence of the individual believed to be God must be in its very nature unlimited, all the ordinary defences of chastity, property, or life must be thrown down. An *imagined* command of God would as easily induce Miss Nottidge to murder her mother as to go to the Agapemone, —to destroy herself as to sacrifice her property. If the whole understanding be not engaged in such a delusion as this, and we firmly believe it to be a profound psychological blunder to say it is *not*, at any rate the unaffected part in such a *partial insanity* must be ever practically useless *quoad* moral control.

But infinitely the most important portion of the charge is the *dictum*, that the fact of being dangerous to himself or others is the test of the propriety of confinement. The whole profession have properly enough taken alarm at this astounding test, and Dr. Conolly has done battle with the learned Baron in a "Remonstrance" that will not be soon forgotten. He has convincingly proved that we must not wait for danger,— but prevent it; that lunatic asylums are for the protection, the comfort, the *cure* of those consigned to them; that they are hospitals for mental maladies, not gaols for savages. Is it possible that the Lord Chief Baron of England was ignorant of this?

We would especially commend to our readers the letter of the Commissioners to the Lord Chancellor, which they will find in the Number of the Journal of Psychological Medicine cited at the head of this article. They state that in the majority of cases they find little difficulty in satisfying themselves as to the state of mind of the patient; but cases of nicety occasionally arise, exhibiting such peculiarities, and differing so decidedly in some respects from all others, that the Commissioners in dealing with them have been unable to lay down any general rule or principle for their guidance. In no case have they decided that opinions, however wild or extravagant, which were common to any class or body of persons, either in reference to religious belief or otherwise, constituted or amounted to insanity. And in no case have they decided that a patient was insane because his symptoms resembled, in a greater or less degree, those of other patients whom they have previously known; *but they have considered each individual case as depending upon its own special circumstances, and have formed their judgment accordingly.* We have italicized this last sentence, because we think it contains a most valuable truth. Every day serves more and more to confirm our opinion that every case must be considered upon its own special merits, almost irrespective of the *dicta* of

physicians or lawyers. This opinion is by no means shaken by Dr. Conolly's "Remonstrance," who has been as successful in opposing the dangerous errors of the Judge, as he has been signally unfortunate in the expression of his own ideas upon the matter. Yet we must confess that this is owing more to the nature of the subject than to any fault on Dr. Conolly's part; for there are certain delicate questions, upon the confines of perilous extremes, which sustain injury in their very handling—cases, the practical management of which is as easy as the statement of the grounds of that management is difficult.

"There are men of rank," says the Doctor, "of minds so ill-regulated and unsound, that, if uncontrolled, they would be ever the associates of the lowest profligates, and frequent the vilest abodes of vice without shame; and women of wealth and station who will drink to excess, and expose themselves to every possible degradation. No one who is so unfortunate as to have in his own family, or among his acquaintance, any person manifesting any of the forms of perverted character above spoken of, will for a moment withhold assent from the opinion that all such persons require to be treated as insane."—p. 7.

It would be great comfort indeed to many a sorrowing family to be able to put into a lunatic asylum a lascivious daughter or an abandoned son, but we must enter our firm protest against the doctrine here stated so boldly, that such are *pro tanto* proper inmates of an asylum for lunatics. We have great pleasure in again bringing before our Irish readers the meritorious Journal to which we have just referred. Its price, the value of its original essays, and the general able manner in which it is edited, conspire to commend it to all lovers of the deeply interesting portions of Medical Science discussed in its pages. In the Number just issued, together with the letter of the Commissioners before referred to, will be found a good report of the trial and a very impartial review of the whole bearings of the case. Dr. Wingett's letter follows on the same side with Dr. Conolly, but its chief object is to show that the Conolly of the "Remonstrance" is not the Conolly of the "Indications of Insanity," and, on the ground of his having had good medical authority for his *dictum* in the very man who now remonstrates with him, attempts a certain sort of defence of the Chief Baron. When we consider that the Court had the Act of 8 & 9 Vict. c. 100, for its guide, and *not* the "Indications," which was published some time *before* that Act, and that no good would accrue to the profession by knowing that a great authority had changed his mind in the course of ten years, we look upon the letter of Dr. Wingett as unnecessary.

China and the Chinese, their Religion, Character, Customs, and Manufactures; the Evils arising from the Opium Trade, with a Glance at our religious, moral, political, and commercial Intercourse with the Country. By Henry Charles Sirr, M. A. 2 vols. 8vo. London, Orr and Co. 1849.

Voyages and works of travels, although not purely medical, are generally deserving of notice in a professional periodical, for the quantity of information they afford on some of the kindred or collateral sciences with which medicine is connected, as natural history, in its old and unlimited sense, botany, materia medica, and popular medicine, &c. It is for this reason that we have introduced "China and the Chinese," the work of our countryman, Mr. Sirr, to the attention of the readers of the Dublin Quarterly Journal. With the general bearing of the work we have but little to do; a large portion of it appeared in the Dublin University Magazine for the year 1846, and these chapters, with large additions, are now published in the two handsome volumes before us. Mr. Sirr has enjoyed ample opportunities of becoming acquainted with Chinese manners and customs, as far as it is possible to penetrate into the arcana of the Celestial Empire. He has used his eyes and looked abroad, and observed and scrutinized, and criticized, sometimes rather sharply, we must confess, upon all he saw and heard. The work contains abundance of information on a country to which we all now look so anxiously, as containing so many of our friends and companions, and concerning the diplomatic management of which by the English governors so much difference of opinion exists. Everything the traveller saw, within doors and without, men and manners, both of colonists and natives, soldiers and civilians, from a war-junk or a Joss-house to the button of a mandarin and the toilet of a lady, are told with an off-handedness, and in some places with a raciness of style that carries the reader along imperceptibly. From a portion of the title of the book we expected to have found some desperate *exposé* of "the evils arising from the opium trade." We at least anticipated finding several chapters exclusively devoted to the destruction of that, so-called, nefarious traffic; but, upon the whole, the owners of the opium clippers, and the cultivators of the *Papaver somniferum*, need not feel very wrathful at Mr. Sirr's tirade against their traffic. With the exception of a few *side wipes* as he passes along, he does not enter much into the subject of the opium trade. There is, however, a chapter devoted to

the knowledge of the healing art, which is worthy the attention of the medical reader, although many of the facts stated in it are already known to those who have examined the reports of the Medical Missionary Society, and those works in which Chinese medicine was treated more at length. But, independently of these circumstances, the readers of the Dublin Journal are not altogether unacquainted with the state of Chinese medicine, as in the twentieth and twenty-third Numbers of our former series were published two treatises, by Dr. W. Lockhart, long a resident in China, one upon midwifery, the other upon inoculation, the great bulk of the materials of both being translated from the Chinese.

"The climate of China is," says Mr. Sirr, "in no part of the empire, salubrious or adapted to the European constitution; but, of all parts of China, Hong-Kong is the most insalubrious, and the variability of the seasons and temperature are exceedingly injurious, and test the strength of the most robust constitutions. It would convey an inadequate idea of the rainy season, merely to speak of the torrents of rain; for the flood-gates of heaven appear to open, and pour forth torrents of water, apparently threatening the earth with a second deluge. The rainy season continues through the months of May, June, and occasionally July, and when these rains cease, miasmata arise, and the most unhealthy season of the year commences. The summer has been admitted, by all the Anglo-Indians whom we met in China, to be far hotter, and more fatally injurious to the constitution, than the hot months in India; and we have found continually the thermometer, standing in the shaded verandah of our dwelling, at 100°. The mortality which has prevailed among our troops, from the first landing in Hong-Kong, in 1840, up to this time, the end of 1848, will fully bear out the assertion; and, in despite of commodious barracks and *hospitals*, our poor fellows fall victims to the pestilential climate of Hong-Kong in a fearful manner. The winter is exceedingly cold and piercing, and a north-east wind blows, whilst a burning sun scorches the head, and the transition, from the burning heat of summer to the cutting blast of winter, injures the most robust. The intense heat of an almost vertical sun, whose rays are reflected by an arid rock, must necessarily prove injurious to health; but when this is combined with nights of piercing cold, when the thermometer falls below freezing point, and water freezes in the ewer placed in a bed-chamber, the effect upon a European constitution must be disastrous. In fact, the winter in China is FELT much more severely than in Russia, where the internal arrangements of the dwellings are such as to exclude all cold; in Russia, warm clothing will suffice to protect those who take exercise in the open air, which it will not do in China, for neither furs nor cloth will exclude the cutting north-east wind, which chills, and seems to penetrate the very *marrow of the bones.* Although, as before remarked, the climate of China is

in no part salubrious, yet the north is the most healthy portion of the empire; but fever and dysentery prevail through the whole land, and the sudden changes from heat to cold produce rheumatic fevers and catarrhs of a severe nature, which too frequently prove fatal.

"The scourge most dreaded in China is the fever termed the Hong-Kong fever, which has proved more fatal than cholera morbus; and at the time we now write, November, 1848, so terribly has this fever prevailed among our troops, and proved so fearfully fatal in its character, that our men were ordered to live on board ships moored in the harbour of Victoria, in the hope of arresting the progress of this scourge. A new symptom has developed itself in this disease, namely, that of an eruption, resembling the small-pox, accompanying the fever, and our most skilful medical officers are baffled in their endeavours to save the victim upon whom the harpy, Hong-Kong fever, has cast a withering glance. The temperate and intemperate become alike the victims of this dreadful fever, which generally commences with slight headach, gradually increasing until the whole head is so tender that no part can bear its own weight or pressure on the pillow without agony. The eye-balls are in such extreme pain, that light can be ill endured, yet the suffering produced by the closing of the eye-lids is intolerable; the frame becomes weak and enervated, and the patient finds himself unable to assume an erect posture, whilst the fever is raging to an incredible degree; the symptoms increase hourly, and the patient usually sinks under its violence about the third or fifth day. Hong-Kong fever has these peculiar features, that the patient will, apparently, become much better, and rally considerably a short time before death; instances have constantly occurred of the sufferer sitting up in bed, or, if allowed, rising from it, when suddenly delirium would become manifest, frequently in a violent degree, and if opposed injudiciously by force, the violence of the patient will increase, until he expires, apparently from exhaustion. In other cases, the patient, after rallying, will be seized with frightful convulsions, the features and eyes becoming distorted, fixed, and rigid, and after some hours of painful suffering (distressing in the greatest degree to the medical attendant, because his knowledge is insufficient to alleviate the agony he witnesses, or save the life of the patient), the stricken being will cease to breathe. If blood be taken from the arm at the commencement of the fever, the case is generally sure to terminate fatally. This fearful disease appears to have baffled all medical skill, for treatment which has proved successful in one case will be ineffectual if adopted in another; and medical men admit that medicine is of little service in this fever. All that can be done is to administer aperients, febrifuges, and apply cooling lotions to the head; local bleeding is also adopted, and beneficially, by the application of leeches to the burning temples. The patient should be kept in a horizontal position, with the head depressed; light nutriment should be constantly given, the utmost kindness,

and most soothing manner used, when delirum appears. A medical man can therefore do but little towards the recovery of his patient, and the only chance of recovery, humanly speaking, is from constant, careful, judicious, and tender nursing, which men can rarely receive in Hong-Kong or China, being generally left to the care of servants or male friends, unhabituated to act the part of nurses."—pp. 6–10.

"The returns to the Army Medical Department will show the fearful ravages from disease, made amongst our troops: the mortality has been, and is fivefold greater, *than has taken place in any other part of the world.* Military men and civilians, who have passed the greater part of their lives in India, have affirmed that they never witnessed, or heard of so much disease, in any part of British India, as they had beheld in Hong-Kong; and others who had fought many hard battles under India's scorching sun, and who had withstood an Indian climate for many years, with unimpaired health, have fallen victims, before our eyes, to the pestilential climate of Hong-Kong, which spares neither the temperate nor intemperate, young or old, strong man, youth, or feeble woman. Animals as well as human beings cannot endure the insalubrity of Hong-Kong: horses which are imported there from India, the Cape of Good Hope, or Australia, are frequently ill, and too often die; and the climate is peculiarly fatal to thorough-bred English dogs. A great favourite of ours, of a most valuable breed, which was brought from England, died in our house, after suffering great agony, exhibiting all the symptoms of Hong-Kong fever; and we have heard of many other dogs that were carried off by the same disease. The only living creatures which appear to defy the climate are vermin of all descriptions; these thrive and multiply to the ruin and destruction of every description of property. The cheerful note of a singing bird, desporting in the regions of air, in the full enjoyment of existence, is never heard in Hong-Kong, and their tuneful notes, except as imprisoned pets, never glad the ear."—pp. 14, 15.

There are some statements in the chapter devoted to the knowledge of the healing art among the native physicians which would amuse some of our medical readers. Among these we may mention the rather lengthy description of the mode of adjusting dislocations of the spine, an accident which appears to be more common in China, and, according to some accounts, attended with less danger, than in this country. Mr. Sirr affords us the following account of the practice of acupuncture:—

"When acupuncture was introduced into China, it is impossible to determine, but the operation has been practised from time immemorial, was introduced from thence into Japan and Corea, and

acupuncture is as much in vogue in the latter countries as in the former.

" The first correct information that Europeans received of acupuncture was in the year 1679, when Ten Rhyne, a medical officer in the East India Company's service, stated that a practice was prevalent in Japan, which was unknown to the Greeks, Romans, or Arabians. It appears that a Japanese soldier, who was appointed by the Emperor of Japan to conduct the English to the palace, was seized with vomiting and pain in the abdomen, after drinking iced-water. He took wine and ginger, but these remedies proving useless, he had recourse to acupuncture, in the presence of Ten Rhyne, popular belief being, that acupuncture allows a subtle and acrid vapour to escape, which was the cause of the suffering. The guard laid himself upon his back, placed the point of a needle on his abdomen, struck the head to make the instrument pass through the skin, then turned it round several times, until the needle had apparently penetrated to the depth of an inch, drew several deep inspirations, and withdrew the needle, pressing the puncture with his fingers, to force out the imaginary vapour. Ten Rhyne affirms that the soldier repeated this operation in his presence four times, when he declared that he was quite well, and perfectly free from pain.

" We have seen this remedy applied to one of our domestics in Hong-Kong, who had been suffering from neuralgic pains in the shoulder: acupuncture was applied to the part affected, two needles being left in his flesh for five days, and our servant declared himself greatly relieved by having holes drilled into his person. The use of the puncturing needles requires extreme caution, lest the operator should wound a blood-vessel; and to prevent unskilful practitioners from adopting this remedy, the following edict was promulgated in China. In the 297th section of the code we read, 'That whenever an unskilful practitioner, in administering medicine, or using the puncturing needles, proceeds contrary to the established forms, and thereby causes the death of a patient, the magistrate shall call in other practitioners, to examine the medicine, or the wound, and if it appear that the injury done was unintentional, the practitioner shall then be treated according to the statute for accidental homicides, and shall not be any longer allowed to practise medicine. But if designedly he depart from the established forms, and deceives in his attempt to cure the malady, in order to obtain property, then, according to its amount, he shall be treated as a thief; and if death ensue from his mal-practice, then, for having used medicine and practised his art with intent to kill, he shall be beheaded.'

" These instruments are used for the purposes of scarification and acupuncture, which is adopted most successfully in the treatment of tumours, to which the Chinese are especially liable. If the patient does not bear the operation of the needle, it is at once withdrawn, but if he does, and the disease proves obstinate, it is

introduced two, three, or more times. The more severe the affection, the deeper must be the puncture.

"There is one practice of the Chinese surgeons which entails too frequently severe suffering upon the patient, we allude to the application of powerful caustics: the sufferers have continually applied to our medical men for relief, cases presenting themselves where small sores originally, by the injudicious use of escharotics, have extended, until a portion of the tissue, and occasionally some important organ, has been either seriously injured or partially destroyed. One of the most powerful irritants used is called the *moxa*, or burning the flowers of the amaranthus upon the skin.

"Bleeding is seldom resorted to, but leeches and cupping are employed when local blood-taking is necessary; venesection is highly disapproved of in fevers, the Chinese practitioners arguing in this wise: 'A fever is like a cauldron boiling; it is requisite to reduce the fire, and not diminish the liquid in the vessel, if we wish to cure the patient."

We have given these extracts in order to interest our readers in works of travel, and to afford information on a subject of great importance at present to young medical men in particular.

In the seventh Number of the present Series of this Journal will be found a full review of Dr. Wise's commentary on the Hindu system of medicine, to which we would refer for a fair exposition of oriental medicine generally.

PART III.

REPORTS, RETROSPECTS, AND SCIENTIFIC INTELLIGENCE.

MEDICAL MISCELLANY.

Letter from Dr. Tuthill Massy, *of Exeter, on the recent Epidemic of Fever in South Wales.*

Sir,—In reading your Report of the Irish fever, it occurred to me that it might not be uninteresting to your subscribers if I give them an outline of that fever as it came into South Wales. Imported into the collieries from the South of Ireland, and landing at Swansea and Cardiff, it made its way to Newport in Monmouth, with the ill-fed poor from Skibbereen, and the labouring classes from Cork, Cloyne, and Kinsale. A cargo of fifty, and now and then one hundred, would escape across the Channel in those open coal ships. On landing they advanced in groups to the relieving officer of the Newport Union, to get tickets for a night's lodging in the Refuge. From this unventilated retreat six, seven, or ten would be forwarded, on the following morning, to the temporary Fever Hospital; as many more issued from the crowded lodging-houses, sinks of sensuality, tottering with famine-fever: they likewise were admitted.

Morning after morning this scene went on, and evening after evening the grave was dug, and the poor wanderer was silently interred, unknown and unregretted.

It will be essential for me to state, that my introduction to this temporary Fever Hospital was through the coroner, William Brewer, Esq., a surgeon of considerable repute.

Half the patients in the hospital (forty) were allotted to me, the other half were under Mr. Hawkins; I had also the care of half the Union hospital, half the town of Newport, and all the poor in that once Roman city, Caerleon, and the district, including the peninsula as far as Goldcliff, bounded by the River Usk and the Severn.

All over this immense district the fever was scattered. My attendance began in May and terminated in November, 1847: during these six months upwards of 6000 Irish got advice.

The thermometer was several degrees above summer heat in May when I entered the barn, not worthy the name of hospital,—a shocking place. The darkness terrified me as I stepped over the beds of those creatures as they lay side by side, moaning and groaning, and those who were able calling for "Water! water! oh! the thirst!" A candle was required to examine the anxious face, the flushed cheek, and the parched tongue, of those who lay in the angles of the building.

According to the reports of the last week thirty had died; yet the barn was full; the places of the dead were quickly taken.

Those many deaths alarmed me. I looked for the cause, and quickly found it in the want of air and light. Next morning the bricks and mortar were removed from four oblong windows at the men's side. One large blind window was opened at the women's side, about sixteen feet from the ground, and then the healthy summer breeze wafted over the wooden partition which separated the male and female wards. The windows and doors were kept open day and night until September.

On the week following this ventilation we had only nine deaths, and the recoveries were increased in proportion. I have kept no notes, so you cannot expect more than a glance. Indeed I had not time to take notes; but I can readily state the number of inhabitants who came under my care with the fever, or were seen by me in consultation.

1. The chaplain; 2. the surgeon; 3. the master; 4. the matron; 5. the nurse (all Welsh); recovered.

6. The gardener to the Union (Welsh); died.

7. A Roman Catholic priest (Italian); recovered.

8. The porter at the hospital (Welsh); died.

9, 10. Two nurses (Irish); recovered.

11, 12, 13. Three prostitutes (Welsh); recovered. Each had gonorrhœa, which did not diminish during the fever.

14. A brothel keeper (Welsh); recovered.

15–19. A family of five (Welsh); all died.

20. The son of the master to the Union; recovered.

21, 22. A man and wife (English); died.

23. A labourer (English); recovered.

24. The nurse to the Refuge (Welsh); recovered.

Of those twenty-four who got the fever from the Irish, five had typhus (Nos. 6, 7, 8, and 22); the others had continued fever of seven, fourteen, and twenty-one days' duration.

A poor Cork woman, after her arrival, and while on her way to the relieving officer, got labour pains, and was delivered in the street. She was put into one of those unventilated black-holes, and died of fever in a few days; her husband and five children quickly

followed her. Those seven persons would not, probably, have died or got the fever if left unprovided for by the poor law.

I stated to the Board of Guardians how they were the cause of this spread of fever, and advised an *open shed* to be erected, for which £30 was granted. This had almost a magical effect in checking the fever; the applications to the hospital diminished.

You see by my Report how very few English and Welsh caught the fever; but it so happened that at Caerleon the Irish who were resident there (some so long as twenty years) only had the fever. Their food was the same as the Welsh and English, but their clothing was light; not one wore flannel against the skin, and flannel, I believe, is a great preventive against fever, keeping out the heat in summer and the cold in winter, equalizing the temperature of the body. A non-conductor next the skin is essential to people living in a climate so variable as our's, one day burning hot, the next frequently piercing cold; and on this account the poor should be told that a woollen shirt is more wholesome than a calico or linen one. Although I do not recollect having seen fever in a Welsh or English family at Caerleon, yet I saw it at Nash; and I must believe the disease contagious, for all our nurses had fever, and any one remaining long near the sick got ill with febrile symptoms, resembling the symptoms of the patient that mostly engaged their thoughts. Those patients who came from a particular lodging house in Friars' Fields were sure to die, all of them getting delirium.

One morning, as I entered the hospital, I saw the priest kneeling by the side of a lovely Irish girl in the agonies of death. I sent him word that he would be very likely to get her fever if he continued so close to her bed. A few days after I saw him with the same disease, the head greatly engaged, fever running high, with paroxysms of delirium, requiring two men in attendance.

Among the comfortable classes head symptoms were most frequent.

The poorer classes had chest and abdominal symptoms; all the starved complained of their hearts,—a feeling of sinking in the epigastrium. One sad, gaunt, tottering young lad cried out, "Oh! doctor, my heart's clean gone," Pains in the legs and back were constant.

We had no well-marked critical phenomena in the cases of starvation, nor had we, during my six months, in any case purpuric spots, petechiæ, or epistaxis.

We had but two cases of dysentery; one died. We had diarrhœa more or less in every case.

We had few cases of relapse, and those few arose from incaution in diet. The least portion of animal food taken into the stomach before the tongue got clean produced a relapse most tedious and difficult to treat.

One person, attended at her home, had bed-sores; and had the

cases been attended to at their homes I do believe the mortality would have been doubled.

The densely populated, badly ventilated, and undrained districts presented all the fever, and, of course, a great mortality.

The duration of the disease varied from seven to fourteen and twenty-one days, but the relapse continued for thirty and forty days. There was nothing remarkable in those primary cases which were subsequently followed by relapse.

There were hardly any modifications of the symptoms in individuals of the same family placed under similar circumstances.

The diversity of symptoms in members of the same family who had contracted fever from the same cause was so slight as not to be noticed.

The usual sequelæ were effusions of serum, first in the chest, next in the abdomen,—both fatal.

In such cases alteratives and absorbents were given. That mild preparation, hydrargyrum cum cretâ, combined with Dover's powder, I found best in checking the diarrhœa. When fulness of the stomach was much complained of, a scruple of ipecacuanha as an emetic, and one hour after a scruple of rhubarb as an aperient was found useful.

Local bleeding was resorted to in some cases of delirium with watchfulness.

Regimen did most good; farinaceous food, with two glasses of port wine a day. I also gave table beer and broth when the tongue cleaned and the stomach strengthened; and lastly, meat. The nurse had camphor mixture to give to those who called for physic.

I treated the cases of collapse, with debility and insensibility, with a pint of hot porter, a warm bed, hot bricks rolled in flannel to the feet, and a large sinapism over the chest and abdomen for twenty minutes. The effect of this treatment was most pleasing; the patient fell asleep, and awoke next day with a warm skin, requiring only to be dieted for a few days.

Lumbago was a constant complaint. Pains in the chest, and flying here and there, were relieved by a mustard poultice,—a most useful application in any shape,—as a stimulant applied largely and for a short time; as a counter-irritant kept on longer; and sometimes I used it to produce vesication.

One remedy I should not omit, and by far the best medicine,—*light* and *air*. A pure atmosphere is a wonderful medicine. I procured its administration here, and in a few hours I saw the oppressed breathing relieved, the livid lips become rosy; the thirst was diminished; the disease was changed: those poor exiles had the dungeon opened.

It is a great oversight that people give their physicians more credit for *curing* than for *preventing* disease, and, indeed, more pay too. Notwithstanding this ingratitude, I must give the preventive remedies their due,—air, light, water, good fires, woollen clothing next the skin, temperance, and three or four meals a day of wholesome food. The medical attendant should always, on leaving an ill-

ventilated fever house, repair quickly to the nearest hill, and clear out his lungs with fresh air.

A drunken porter had fever four times, and a nurse twice.

We had some cases complicated with small-pox. One little girl had been vaccinated; a few days after fever set in; she got confluent small-pox with the cow-pox, but yet made a rapid recovery. This case agrees with Jenner's views, who said, in a letter to Lord Egremont: "I do not consider them as two distinct diseases, but as *varieties* only of the same disease; and therefore they might co-exist in the same constitution, and that shows a mixed disease had been produced."

I have just read the following account of the Irish poor in the *Monmouthshire Merlin:* "The number of those homeless outcasts who found temporary shelter in the Refuge of Newport, between the 31st December, 1847, and the same date, 1848, was no less than 12,505!" This was the year after I left that place: it is a pleasure to know that they have now an open shed and a ventilated Refuge. Had the editor of the *Britannia* read your Report of the Famine-fever and the treatment adopted by the Irish physicians, he never would have written the following: "But while the doctors are busy in writing their prescriptions, the wretched patient continues to waste away, and each new remedy she takes only aids the fearful progress of exhaustion."

I have the honour to be, &c.,

R. Tuthill Massy, M. D.,

Physician to the Exeter Dispensary.

May 16, 1849.

To the Editor.

Case of acute Glanders from Infection. By Dillon Kelly, Esq., Mullingar.

James Donnelly, a carman, ill since the 23rd of August, when he first complained of chilliness followed by headach, thirst, and loss of appetite, for which he took some aperient medicine, and continued to work on till the 25th, when he was obliged to lie down.

When I first saw him, on the 27th, he complained of intense pain in the lower portion of the left side of the chest, of so excruciating a character, that the slightest motion was all but impracticable, respiration now being performed with the greatest difficulty. Dulness on percussion, but no frottement or egophony could be detected; cough; hot, dry skin; headach; pulse 120, full and hard; tongue foul and covered with a tenacious whitish paste; thirst: passes water freely.

Considering the attack, from the great constitutional disturbance accompanying it, one of pleuritis, although some of the pathognomonic signs of that disease were not present, I took about sixteen ounces of blood from the arm, with marked relief (blood buffed but not cupped), and gave half a grain of tartar emetic every second hour, and five grains of calomel at bed-time.

29th. Pain in side and head have completely disappeared; cough all but gone; pulse 108, soft and compressible; skin still hot; complains occasionally of acute pains shooting through the right hypochondrium, with much distress and restlessness at night; thirst still considerable; white, foul tongue; hot skin: tartar emetic to be continued, increasing the intervals to four hours; five grains of calomel at bed-time, washed down with a quarter of a grain of acetate of morphia in one ounce of camphor mixture.

31st. Complains to-day of pain of so excruciating a character, in the left natis, accompanied by such excessive sensibility, that the slightest motion could not be attempted; the least pressure even over the seat of soreness, which was confined to a circumscribed space not more than one inch in diameter, midway between the acetabulum and posterior superior spine of the ilium, would cause him to scream out in intense agony.

Viewing the attack now in the light of acute muscular rheumatism complicating neuralgia, I had recourse to firing (the state of his pulse contra-indicating even topical bleeding), and with the most happy result, the pain ceasing as if by magic. Fifteen drops of colchicum wine were given every fourth hour, and a morphia draught at bed-time.

September 1st. Had a very good night; the pain, however, again returned during the morning, but not with the same degree of intensity; there is now, for the first time, a tendency to diaphoresis, which in this case is destitute of the characteristic acid smell of the rheumatic secretion, a just perceptibly fetid odour developing itself in its stead; bowels natural; passes water freely: a blister to be applied over the seat of pain, and the colchicum and morphia repeated.

2nd. Much relieved: blistered surface to be dressed with mercurial ointment, and the colchicum to be continued.

4th. Says he feels greatly improved; perspired copiously during the night; pains have ceased.

6th. Had a most restless night, from retention of urine, which required the introduction of the catheter; the emptying of the bladder, however, did not entirely remove the restlessness; tongue still foul, and darker than it was, but moist; bowels regular; quite free from pain: half a grain of acetate of morphia and one ounce of camphor mixture at bed-time.

7th. A small accuminated pimple, surrounded by a crimson areola, made its appearance during the night, over the right cheek, which at the evening visit had assumed the appearance of a pustule, surrounded by an irritable-looking darkish red circle. The cheek of the opposite side was occupied by a reddish brown tumour, resembling a patch of erythema nodosum with erysipelatous inflammation; a similar tumour having developed itself on the back and inner aspect of the left hand, and an incipient one on the dorsum and inner aspect of the opposite foot.

These being most alarming appearances, and generally indicative of specific disease, led to the inquiry whether or not his horse had

lately any running from his nose; being answered in the affirmative, the disgusting-looking malignant pustule of acute glanders was before me, and the anomalous symptoms of that frightful disease, so long lurking in the system, at once revealed: punch, beef-tea, acetate of morphia, and Mindererus' spirit, was the treatment adopted under these circumstances.

8th. Had a quiet night from the anodyne; face, neck, and extremities now studded over with minute pustules, having all the appearance of varicella, each surrounded by a crimson areola; conjunctiva of left eye, immediately within the outer canthus, the seat of a similar pustule; tongue whitish brown, and loaded, but moist; no sordes on teeth; no impairment of intellect or delirium; skin perspiring copiously, the secretion developing a strong acid reaction on the application of litmus; bladder has recovered its powers; urine rather copious, taking the profuse diaphoresis into account, orange-red, transparent, sp. gr. 1026, acid, showing slight traces of bile, no albumen or urea, but uric acid in abundance.

9th. The minute pustules of yesterday have become exceedingly disgusting in appearance; purulent vesicles on the face and neck, whereas those on the extremities have completely disappeared; pulse 120, weak and thready; tongue a little darker near the tip, but still moist; no nasal discharge, but slight stuffing in the posterior nares; rather copious expectoration at times, when he says it comes up in mouthfuls; two small, hard, painless, and colourless tumours have developed themselves, one on the outer aspect of the arm, a little below the insertion of the deltoid, the other on the fore-arm, over the upper third of the supinator longus; no delirium or confusion of ideas.

Dr. Stokes, of this town, having seen him with me at this stage, suggested, that as the symptoms in the commencement appeared so much improved by the mercurial treatment, recourse should be had again to it, hopeless although the case was, and advised a trial of the oxymuriate. A table-spoonful of a mixture consisting of two grains of the oxymuriate to eight ounces of water was directed to be given every fourth hour, and his punch and nourishment to be continued.

6 P. M. He is now bathed in a most profuse and slightly fetid perspiration, exhibiting a strong acid reaction with litmus; a few pustules in a transition state of development are unequally scattered over the upper extremities; pulse 160, weak and thready; tongue moist; intellect unimpaired. Some of the pustules on the face have now assumed the appearance of those produced by tartar emetic, more especially two large ones on the malar tumour, which is considerably increased in size, and of a shining dusky red colour, and, together with the conjunctiva of the eye on that side, which is in a state of transparent amber-coloured chemosis, and so exaggerated as to separate the swollen lids, protruding to fully the size of a small currant at their outer angle, gives to the countenance a most sickening and truly frightful appearance.

Since morning the tumours on the arms have become tense and elastic, but without fluctuation, and two more have during that interval developed themselves on the left leg, one over the gastrocnemius, and the other immediately under the insertion of the ligamentum patellæ. He has passed water freely during the day, presenting the same character as that of yesterday, but with increased specific gravity, which to-day is as high as 1033; it preserves its transparency on cooling, and on the addition of one-third of nitrous acid assumes a dirty greenish purple colour, also transparent.

11th. Died at 5 A. M.: intellect unimpaired to the last moment.

It had for a length of time been a *questio vexata*, whether the virus of glanders was or was not infectious; most veterinarians holding it was, strictly speaking, contagious, and moreover, that actual contact of the diseased secretion with an abraded surface was absolutely necessary for its propagation.

More recent and more extended inquiries have, however, proved the fallacy of those opinions; and the profession now may generally believe in the infectious nature of the disease, an hypothesis, the correctness of which the history of the present case goes a great length indeed in confirming, as in it we have positive evidence of the propagation of the disease, not by contact with an abraded surface, but purely by venous absorption, probably through the medium of the respiratory apparatus.

In this case there was no abrasion of the cuticle, no appearance of sore, pustule, or vesicle, on any part of the patient, so far as he could remember, prior to the attack; no recollection on his part of ever having wiped his hands or face with any cloth used about the horse, or of ever having drank out of the same vessel; on the contrary, he merely drove him and worked with him, his wife and children generally making up the animal at night, feeding him, and harnessing him again in the morning.

The unfortunate subject of this case was, like a great many of his class in Ireland, obliged to keep his horse in the same habitation with his family, which consisted of but two rooms, one an L-shaped apartment containing the kitchen and stable, the other his bed-room, a miserable hole about six feet by seven, separated from the stable by a four-inch partition broken through in three or four places, and presenting at its junction with the main wall of the house a fissure extending from the floor to the top of the partition, and increasing in width from below upwards, where it attained fully the extent of four inches.

In the bed-room, immediately opposite and quite close to this fissure, the head of the unfortunate man's bed was placed, the bed being parallel to and in close contact with the partition; and on the opposite side or stable was the crib from which the horse was fed, and to which he was tied at night.

A paved channel separated the stable from the kitchen, which miserable attempt at cleanliness made the stable *proper* rather too short for a horse of any size, who, to avoid lying on the pavement,

litter not being very abundant, took at night a diagonal position, and as he was tied to the right, his head was brought into immediate contact with the angle where the fissure was, as was quite evident from the stains on the main wall and partition, immediately close to which, and on the same level, slept his master, who, by this unfortunate arrangement, must have been inhaling, night after night, the air expired by the animal, loaded with the virus of glanders.

These are facts not only curious in themselves, but which, in my opinion, go a great way in proving the infectious nature of the disease; and I give them to the profession, in so far as they add another case in proof of the correctness of that hypothesis; at the same time trusting the publication of this and similar cases may at length arouse the Legislature to the necessity of placing glandered horses under the surveillance of the police.

A Case of Poisoning by Laudanum, in the advanced Stage, successfully treated by Bleeding. By James S. Hughes, M. D., F. R. C. S. I., Surgeon to Jervis-street Hospital.

I was summoned to see (in conjunction with the late Dr. Kerin, and Messrs. Daly and Gorman of Henry-street) a gentleman in the Londonderry Hotel, who was supposed to be dying of apoplexy. We found him lying in bed, on his back, in a state of insensibility, with stertorous breathing; pulse extremely rapid, scarcely to be felt; face leaden-coloured; lips purple; pupils minutely contracted and fixed; extremities cold, and covered with a clammy perspiration. On throwing down the bed-clothes to examine him more minutely, a half-ounce empty bottle, labelled "Laudanum," fell on the floor, and the only information we could obtain was, that the patient had spoken to one of the waiters on that morning, between nine and ten o'clock, at his bed-room door, dressed in his night-shirt, and that, suspicion being excited, at five o'clock the door was forced open, and he was found in bed in a state of insensibility. The stomach pump was immediately resorted to; the tube being introduced, an attempt was made to withdraw any fluid contained in the stomach, but it having been ascertained that there was no fluid in that viscus, water was injected into it, and withdrawn, in order to remove any solid material, if present; the fauces were irritated, and a twenty grain solution of sulphate of zinc was injected through the tube, and repeated after a short interval, without producing emesis. Sinapisms were applied to the calves of the legs, together with heaters and turpentine frictions to the extremities, by which means the temperature was slightly elevated, and the pulse became fuller. Cold affusion to the head was next resorted to, with a view of taking off the pressure from the brain by unloading the vessels, which were turgid, but without effect. The patient appeared to be sinking, the coma becoming more profound, and the breathing more and more stertorous. On considering the

history of the case, and the probability that the laudanum had been swallowed immediately after the patient had returned to his bedroom in the morning, and that, if so, full time for the absorption of the poison had elapsed, and viewing the patient as sinking under cerebral congestion, as evidenced by the great turgescence of the vessels of the head and neck, together with the stertorous breathing, we determined to try the effects of blood-letting, and for this purpose the left temporal artery was opened, the blood from which at first merely trickled down, being very dark-coloured and thick, but after a few seconds a jet of arterial-coloured blood sprang forth, *immediately* on which the patient gave a deep sigh. Vomiting, with the expulsion of a small quantity of fluid containing a little indigested matter, devoid of taste and smell, and mixed up with mucus, ensued; his pulse became fuller, and less frequent; sensibility returned, and his countenance rapidly improved. We had him then walked up and down, between two stout porters, through the long corridors, the men being relieved from time to time, for we found it of vital importance to keep him in forcible and active motion, for if he was left to himself for a moment, stertorous breathing and a state of somnolency ensued. Brandy punch and hot coffee were given to him at short intervals. The drowsiness continued for over twenty-four hours after he came under our observation, during which period he was kept in a roused state, and the stimulants were continued.

This gentleman perfectly recovered, and when convalescent made the following statement. He said that, some time before the attempt to poison himself, he had determined to commit suicide, provided he did not receive a favourable answer to a letter which he had addressed to a high official officer in this city; and that, with a view of carrying out that intention, he went to a house in Sackville-street, where he asked for one ounce of laudanum, but that, as he admitted that he was not in the habit of using it, the gentleman he addressed very properly refused to give it to him. He passed on to a shop in an adjoining street, and, being then on his guard, said he wanted half an ounce of laudanum, and told the shopman that, as he was in the habit of taking it, he wished to have the strongest in the house: the man, not suspecting anything wrong, gave it to him. He then returned to his hotel, went to bed, and having gone out on the following morning, in his night-dress, to the landing at his bed room door, to inquire after the expected letter, and having been informed by the waiter that no such letter had been left at the hotel, he went back to his room, and, having locked the door, returned to his bed and swallowed the half-ounce of laudanum, after which he soon became insensible. Thus upwards of seven hours must have elapsed, from the time of taking the poison, till the alarm was given. In a physiological point of view, the result of the abstraction of blood was highly interesting, for the instant the congested brain was relieved, and thereby enabled to

receive impressions, and not till then, vomiting ensued, the result of reflex action, the stertorous breathing ceased, and the patient was restored to his senses. From the details of this case there can be no doubt that, by the timely use of the lancet, the patient was snatched from the most appalling of deaths, viz., that by suicide.

That the propriety of having recourse to bleeding in the advanced stage of poisoning by opium, when accompanied by symptoms of cerebral congestion, is based on scientific principles, and has the sanction of the highest authorities; it is only necessary to refer to the works of Orfila, Christison, and Taylor; but as I know, from the treatment of more cases than one of poisoning by opium in the advanced stage, which have come to my knowledge, and in which there was evidence of congestion of the brain, that the value of this measure has been occasionally overlooked, principally from the fear of still further promoting absorption, I have been induced to record the foregoing case, which occurred some time since, as it illustrates the marked success which may follow the adoption of blood-letting in *suitable* cases of poisoning by opium, or its preparations. That depletion does favour the process of absorption in the early stages of poisoning is now generally admitted; but we must hold in mind, that a time may come when absorption has been carried to its full extent, and when the patient is sinking from congestion of the brain; and it is in this particular stage, when after emptying the stomach of its contents, that blood-letting is not only justifiable, but imperatively demanded. Dr. Christison, when alluding to the treatment of poisoning by opium, says that "venesection has been recommended, and successfully used by some physicians. If the stomach be emptied, and the patient kept roused, as may almost always be done when means are resorted to in time, venesection will be unnecessary; however, when the pulse is full and strong, it may be prudent to withdraw blood, and it certainly appears that, in most cases where this remedy has been employed, the sensibility began to appear almost immediately after." And Mr. Taylor, in his recent work on poisons, when referring to blood-letting in poisoning by opium, states that "bleeding should not be resorted to until *after* the poison has been completely removed from the stomach, as the abstraction of blood acts injuriously by tending to promote absorption; it is indeed only justifiable when, during the second stage, there is a strong, full pulse, with symptoms indicative of cerebral congestion. If the pulse should become feeble and sink after the abstraction of a small quantity of blood, the operation should be carried no farther." The following case of poisoning by laudanum, in which bleeding was followed by immediate relief, is so full of interest, that I think it worthy of a place here. It is published by Dr. Richardson, in the Edinburgh Medical and Surgical Journal, vol. xvii. p. 226:

"An unfortunate female, when in a carriage with a friend, swallowed laudanum; the action was observed, and she was conveyed by

her terrified companion, in about half an hour afterwards, to the house of the practitioner under whose care the author studied: she was taken out in a state nearly insensible. Ten grains of vitriolated zinc were instantly administered without relief; ten more were added in a few minutes afterwards, and other means were in vain resorted to, to excite the action of vomiting, as the comatose symptoms continued evidently to increase; a vein was opened, and nearly twenty ounces of blood taken away; sense and motion *immediately* began to return, the emetic action of the zinc was produced, and the patient was left in a short time afterwards with no other remaining symptom of the danger she had escaped from, than a little languor"(*a*).

Cases in Surgery. By T. Purefoy, M. D., Cloughjordan.

Remarkable Case of Obstruction of the Bowels.—A healthy little boy, aged 6, on the 20th of August, complained of pain in the abdomen, which returned with severity at short intervals. The occurrence of pain was soon followed by vomiting of bilious matter, mixed with food, but without any relief of the pain. An emetic was now given, and subsequently a purgative; the emetic operated freely, but the purgative did not move the bowels. The patient was first visited in about thirty hours after he had been taken ill. The abdomen was then swollen and tense, but not decidedly tender; tongue soft, and thickly coated; pulse 90, and compressible; fever, thirst, obstinate constipation, and irritability of the stomach, so that both fluids and solids were quickly rejected from this organ by vomiting. The urine was of a natural appearance, and the intellect remained quite sound. The treatment consisted in the detraction of blood, followed by the use of the warm bath; at the same time that unremitting efforts were made to remove constipation, and unload the bowels. For this purpose, calomel with opium, croton oil, sulphate of magnesia in solution, and other purgatives, were successively tried, and, as a forlorn hope, the tobacco enema; but all proved utterly unavailing, as the lower portion of the great intestine alone was emptied of its contents. The poor little sufferer lingered for nearly a week in extreme agony, and finally sunk, apparently rather from the effects of exhaustion than in consequence of the existence of inflammation within the abdomen.

Post Mortem Appearances.—Stomach and intestines much distended by very fetid gas. Mucous membrane of stomach pale, soft, and readily reduced by the finger to a mere pulp. The convolutions of the small intestines were glued together by recently effused lymph, particularly in the iliac regions, whilst the lining membrane was

(*a*) For other cases on record illustrative of the advantages to be derived from bleeding in poisoning by opium, I would refer particularly to those by M. Castara, *Journ. de Chimie Med.*, Mars 1831; M. Jul. Fontenelle, *Revue Med.*, Sept. 1829; and Mr. Ross's case, Edin. Med. and Surg. Jour., vol. xix. p. 247.

throughout in a state of congestion. The appendix vermiformis was found of an unusual length, and to have formed a complete stricture on the ileum just where it entered the cæcum, the colon throughout its whole extent being empty, and completely collapsed. The appendix vermiformis was thickened considerably by a coating of lymph, of a deep bluish tint when the lymph was removed, and had made by its pressure a livid-coloured circle quite round the ilium at the seat of stricture.

This case is of interest, in showing that certain mechanical causes may exist during life, capable of effecting a fatal obstruction of the bowels, and yet be beyond the reach of art, either in the way of ascertaining their existence, or of obviating their effects. Fortunately such a case as this just now recorded is of rare occurrence, and will most probably, wherever it exists, destroy life; the fatal termination will of course be much expedited by the undue or incautious use of purgatives. I have no doubt that if purgative medicines are given in these cases without any effect, which, under ordinary circumstances, would produce a free action of the bowels, we may correctly conclude that the circumstances of the case are extraordinary,—that some mechanical obstruction must exist,—and that therefore the further use of purgatives may do harm, but cannot do any good.

Incarcerated Inguinal Hernia.—A healthy labourer, aged 60, who had been the subject of hernia for many years, which he had never found any difficulty in reducing, became suddenly ill one evening, after his day's labour, in consequence of being unable to return the protruded portion of intestine.

At visit he was found lying upon his back, the abdomen tympanitic, tongue coated, pulse moderately full and frequent, whilst there existed a tumour in the groin, of the size of a hen's egg, tense, elastic, somewhat moveable, and tender under pressure. Warm baths and other auxiliaries not being attainable in his wretched cabin, the taxis was perseveringly, but unavailingly, employed for a considerable period of time, when the increasing tenderness of the tumour rendered the continuance of the operation impracticable. The bowels being constipated, two pills, containing calomel and colocynth extract, were given, followed by turpentine enemata, every third hour, to free the bowels. On the following morning, at 9 o'clock, he was found in the same state, the symptoms more severe than on yesterday, as he now vomited occasionally, whilst the bowels remained obstinately confined. As a last resource, he was now placed upon his hands and knees in bed, whilst a long flexible, gum-elastic tube was introduced into the rectum, with a hope of getting away the flatus by which the abdomen was much distended; but the manœuvre did not succeed. Next, a large basin of tepid water was brought to the bed-side, the greater portion of which was injected into the rectum. At this period, a very urgent straining effort coming on, he was made to resist it as long as possible; and when ultimately allowed to relieve the bowels of

their load, he passed a large quantity of feculent matter, together with an unusual amount of flatus, which gave him immediate and decided relief. The taxis was again immediately resorted to, upon his returning to bed, when the empty and flaccid intestine was returned into the abdominal cavity without further delay or trouble. The old man is yet in the land of the living.

There can be little doubt that, in this second case, the incarcerated intestine would very soon have become strangulated, had not the intestine been relieved from the pressure of the stricture; and it is equally certain that the water injected by the rectum effected this desirable object, by stimulating the peristaltic action of the whole tract of the intestines, thus causing such a discharge of flatus and feculent matter, that the distended knuckle of intestine, which formed a tumour quite too large to re-enter the abdominal ring, was now rendered so small and flaccid as to be easily returned into the abdominal cavity. It was a lucky experiment for the patient, as, exclusive of its being *perfectly harmless*, it rendered a surgical operation unnecessary, which but a little before appeared to be almost inevitable.

On the Use of Bandaging in relieving the Cramps in Cholera. By Evory Kennedy, M. D.

The habit of applying a knotted handkerchief as a tourniquet over the spasmodically contracted muscles in cases of cramps, and always with instantaneous relief, led me to test the efficacy of bandaging, many years since, in a case of universal cramps occurring as the result of a violent sustained effort to maintain his position on horseback in a person who had been run away with. Every expedient had failed, the tourniquet only giving partial relief, as the muscles of the trunk were engaged equally with those of the extremities. Bandaging each limb separately, and eventually the trunk, was attended with immediate relief. The recollection of this case induced me to try its efficacy in the cramps of cholera, and with such results as justify me in calling attention to it, as it not only accomplished the object had in view, but also succeeded in keeping up the temperature of the limbs more satisfactorily than by means of friction. Those who have practically dealt with the worst cases of cholera know but too well the difficulties attending the attempts to keep up the heat of the extremities by any or all of the plans at present in operation;—the copious clammy perspiration constantly exuding from the surface, the evaporation of which tends to lower the temperature; the exposure unavoidable in keeping up the friction adding to the chill; the impossibility of keeping up artificial heat so universally over the limbs and trunk as to maintain an equable temperature. The further objections against depending upon these means, from neglect on the part of attendants, and restlessness, jactitation, and exhaustion on the part of the patient, render it of importance to effect our object by some means unat-

tended with such grave objections, or at least to have at our disposal an additional method which may be used as auxiliary to these. Now, I imagine bandaging affords us this desideratum. The bandages should be made of new flannel, cut into stripes of two inches broad; should be applied from the toes and fingers up to the groins and axillæ, as tight as can be borne without inconvenience. The copious perspirations, instead of evaporating, become absorbed into the flannel, and act like a continued warm fomentation, the superincumbent bed-clothes assisting our object by preventing evaporation and the abstraction of the animal heat of the body; whilst our efforts to sustain the temperature are still further aided by placing hot bottles, heated bricks, salt, &c., in contact with the extremities and surface. The disturbance of the patient precludes the application of a bandage over the trunk, similar to that of the extremities. A very simple expedient, however, is supplied in a piece of flannel, or an under-blanket, about a yard and quarter long by half a yard broad. This should be torn down the centre for about a third of its length, and across the remaining portion ten or twelve stripes of flannel, a yard and quarter long by two inches broad, should be sewn. This should be slipped under the patient's back, drawing the two torn ends over the shoulders and down on the chest, and then the many-tailed bandages applied across the chest and abdomen, alternately over-lapping each other, as tightly as the patient can bear without inconvenience. This form of bandage has the advantage of being easily removed and re-applied when it becomes an object to make stimulating applications to the abdomen. The bandages may, however, be rendered available in making our applications, whether stimulating or anodyne, without removal, by pouring any fluid upon them, as they absorb it and keep it in close contact with the surface. This, however, requires to be done with care, else too great a degree of irritation might be induced by allowing the bandages charged with stimulating applications to remain too long in contact with the surface.

Anxious to test the utility of bandaging in cholera upon a large scale before calling the attention of the profession to it, I requested Dr. Travers, physician to the Kilmainham Cholera Hospital, to make trial of it, and give me the results. This he has been good enough to do. His report is subjoined:—

"*Kilmainham Cholera Hospital, 27th Sept.*, 1849.

"Dear Sir,—I have made several trials of your system of bandaging, and in every case had the satisfaction to observe that the effect was beneficial. Before I had seen your paper, I had frequently used a broad ligature round the affected limb, but generally with but little benefit beyond a temporary relaxation of the spasms. In one case, that of a child, your bandage was objected to, but in every other in which it was used either by me or by the resident medical officer, the results were encouraging. I by no means wish to exaggerate the advantages of your plan, or to repre-

sent it as a specific for the symptoms it is intended to relieve, but as an assistant in the treatment of a most intractable disease, it forms a most valuable addition to our resources.

"I remain truly your's,

"Robert Travers.

"*Dr. E. Kennedy.*"

Case of Apoplexy associated with Calcareous Deposits in the Communicating Artery of the Circle of Willis. By Henry Carline, Esq., Assistant Surgeon of the 6th Dragoon Guards.

So few cases of apoplexy occur in which the cause of the extravasation of blood can be distinctly traced after death, that I consider the following worthy of being recorded. It is also an example of the formation of calcareous matter in the arterial system of the brain, at a comparatively early period of life.

H. M'C., aged 29, a stout Irishman, by trade a shoemaker; five feet ten inches and a half high, with dark complexion, hazel eyes, and dark brown hair, enlisted 24th August, 1840, and has been seven times in hospital since then, viz.: in 1840, for eleven days, with diarrhœa; in 1841, for thirty-six days, with venereal (gums affected); in 1842, for two days, with colic; in 1845, for twenty days, with cynanche tonsillaris; and again, in the same year, for three months, with pneumonia; in 1847, for seventy-two days, with venereal (gums affected); in 1848, with venereal; and in April, 1849, for five days, with slight contusion of the right side of the forehead, and a slight cut on the eye-brow.

From the statement of this man's comrades, it appears that he was cleaning his appointments at a quarter to 9 o'clock in his barrack-room, on the evening of the 25th of July last, when he suddenly staggered, and fell upon his left side. On being raised up he complained, in an altered voice, of having hurt his left knee against the form. He was put to bed, when he vomited a good deal, and said that he could not use his left arm or leg. At 9 o'clock he was reported sick, and, when visited, complained, in a stammering voice, of a desire to vomit, though he could not throw anything up, and of pain over the right eye-brow: he was immediately removed to hospital.

The countenance was now very plethoric; he was insensible, frequently picking the right nostril with his right fore-finger; the the breathing was stertorous; pulse intermitting, 45 in the minute, very full and hard; pupil of the right eye much dilated; that of the left contracted: the skin was cold and slightly clammy. He was bled to fifty ounces; as the blood flowed the intermissions in the pulse occurred at longer intervals, until at length it rose to 100, very soft, and free from intermission. Bags of hot salt were applied between the thighs, and bottles of hot water to the feet; blisters to the neck, chest, and calves of the legs; the hair on the head closely clipped, and cold evaporating lotions applied. He rallied slightly

for an interval of half an hour, when the pupil of the left eye was much dilated, but soon resumed its contracted state. At midnight the stertorous breathing was much less; pulse still continuing 100, tolerably full and soft, and the skin of its natural temperature. At this period a creeping motion was noticed to pass through the whole body, at intervals of five or ten minutes; he frequently used both the left arm and leg, and was occasionally convulsed, with bloody, frothy mucus exuding from the mouth. He lingered until twenty minutes before 2, A. M., 26th July, when he expired, without having been at all conscious since admission.

The *post mortem* examination was made thirty-one hours after death. The body was very stout and muscular; slight decomposition had taken place. On removing the calvarium, great congestion of the vessels of the surface of the brain was apparent. On the right side the communicating artery of the circle of Willis contained several small calcareous deposits, varying in size from that of the head of a pin to that of a grain of wheat. The lateral and third ventricles of the same side were filled and distended with a large clot of black blood; and some fluid black blood and bloody serum were found in the left lateral ventricle: the substance of the brain, in other respects, was perfectly healthy. *Chest.*—There were some firm adhesions anteriorly between the pleuræ of the right side, evidently of long standing; the lungs were perfectly healthy, but the heart was somewhat hypertrophied; the valves were healthy. *Abdomen.*—A small mass of scrofulous tubercles was situated in the lower edge of the great lobe of the liver; the other viscera were perfectly healthy.

Case of recent Dislocation of the Hip reduced with great Facility by the Aid of Chloroform. By W. R. Wilde, F. R. C. S. I.

The following case is only worthy of notice from the decided influence which chloroform exercised upon it. Upon the 7th of the present month, a gentleman in the prime of life, of strong make, great muscular strength, and in perfect health, while walking across a lawn some miles from town, about half-past ten o'clock at night, fell into a sunk fence from the upper side. He was immediately conscious of having experienced some great shock, and said his leg was either broken or dislocated, as, upon attempting to rise, he found it impossible to put the limb upon the ground. An easy inside car was immediately brought to the spot, and on it he was conveyed to town with scarcely any pain. I saw him about half-past 11 o'clock, and found that he had dislocation of the hip upon the dorsum of the ilium. The only usual symptom of that accident which was not well-marked was, that the toes did not rest upon the dorsum of the right foot. As he was stripped and sitting upon his bed when I arrived, some slight efforts were made, for a few minutes, to reduce the limb, but without effect. I immediately procured the valuable aid of Mr. Ellis; and two gentlemen who resided in the house, together with a small servant boy, were all the assistance which we could

then (past 12 o'clock) procure. Having placed the patient lying upon his back upon a narrow lounger with a short arm, the usual extension and counter-extension were made, and kept up, as far as the force of our few assistants permitted, for about a quarter of an hour, without the least impression upon the distortion. Mr. Ellis and myself then agreed to try the effects of chloroform, as it was quite manifest that, without the usual mechanical contrivances, or very many more assistants, it would be impossible to reduce the limb, and it would be unwise to allow it to remain unreduced in so powerful a person as our patient until morning. Chloroform was immediately procured, which was commenced to be inhaled about 1 o'clock, two more assistants (boys) having been obtained, so that three persons held the limb and the sofa firmly by means of a sheet passed obliquely across the pelvis and over the groin, in the usual manner, while the other three were ready to make the extension. I administered the chloroform in a full dose, and the patient was very soon under its influence; the extension then commenced, and was kept up steadily, Mr. Ellis himself taking charge of the limb and making the proper rotation. In about four minutes after the patient was in a perfect state of anæsthesia, the dislocation was reduced with an audible noise, and upon the patient awaking from the condition of insensibility, he stated his total unconsciousness of what had occurred, and his perfect freedom from pain or uneasiness. He suffered no further inconvenience, and experienced no ill effects from the exhibition of the chloroform.

This case I think worth recording, as the dislocation certainly could not have been reduced, except for the aid of the chloroform, particularly at so late an hour of the night, and when the usual assistance could with difficulty be procured.

Observations upon Intermittents. By William Harvey, M. D.

[Read at a meeting of the Dublin Medico-Philosophical Society, 1st August, 1782(*a*).]

There is no disorder for which we have so many remedies as an ague, or in which more quack medicines have been employed, a sure proof that there is something still incomplete and imperfect in the present ordinary method of treating intermittents. I do not flatter myself with being able to supply these defects, but I conceive it to be the duty of us all, in a society composed for medical and philosophical improvement, to hazard conjectures that may set us a thinking, and be confirmed or rejected by future observation

(*a*) Extracted from the Memoirs of the Dublin Medico-Philosophical Society, vol. iii. p. 236, to which we referred in the Preface to the first Number of the present Series of this Journal. We have printed this Paper *verbatim* from the manuscript, and we sincerely wish the MSS. of medical men in the present day required as little correction. Dr. Harvey was, as our Irish readers are aware, the chief Physician in Dublin for many years; ague was of very frequent occurrence in Ireland in his time.

and experience. Not to waste more of your time in preface, I shall offer you some remarks on regular and irregular intermittents.

By the first, I mean such as are truly marked by regular paroxysms; by the second, such as where the paroxysms are anomalous, and the patient never has a complete intermission, but has a constant feverishness, and a swollen, jaundiced, cachectic appearance. This state is what the common people of this country call " a dead ague," meaning such as does not show itself in active, violent paroxysms, but leaves a constant heavy languor and sluggishness in the system.

In the first place, I would remark that all agues where the three stages of the paroxysm are complete have a natural tendency to cure themselves, and in many cases the physician need only be a spectator. This I have observed chiefly in tertians, and was formerly noticed by the ancients, who have remarked that the seventh paroxysm often terminated a tertian ague. Since the discovery of the Peruvian bark, and our more intimate acquaintance with the disorders of hot climates, we have, I apprehend, become too anxious to stop the recurrence of the paroxysms, by pouring in the bark too early, supposing that the fits had no tendency to remove themselves. I found, first by accident, and since by repeated observation, that where agues have been stopped prematurely they are apt to leave a state of listlessness and languor, degenerating into cachexy, that is only to be cured by nature and art exciting again new paroxysms. Hence the common people in this situation are rejoiced to tell you that they *shook out*, that is, that this anomalous state is changed to regular paroxysms.

In the treatment, then, of *regular intermittents*, I would recommend our not being in such a hurry to exhibit the bark, but to give four or five vomits at least, each a little before the accession of the paroxysm, and then to administer the bark in large quantities, in the dose of a drachm every hour during the intermission, beginning before the sweating stage has entirely ceased. I have found it useful to repeat the vomit at the time that the paroxysms should have come on, though the recurring of the fits has been apparently stopped; and it will be found advantageous to keep the bowels regularly open.

We may always be more encouraged to commence the administration of the bark where the vomits bring up or down large quantities of bile; for, whatever be the cause of ague, it is its nature to be particularly connected with the biliary secretion. Hence a common phrase among the poor people, that they have thrown up the ague, that is, they have found by common experience, where they vomit a large quantity of bile, the intermittent is less apt to return. The vomits, therefore, most useful are antimonials, as these emulge most powerfully the biliary system.

The practice, then, to be observed in regular intermittents consists in the constant administration of an antimonial vomit a little before the accession of the fit, plenty of diluents during the parox-

ysms, and large quantities of the bark in the intervals. We need not, however, be in great haste to exhibit the bark in this climate, as we shall find it more effectual after four or five paroxysms have passed, and that our patient has been plentifully emptied upwards and downwards. If this last regulation was carefully complied with, we should, I believe, seldom see such violent paroxysms as threaten the life of the patient, and oblige the physician to throw in the bark at all adventures, to obtain, if possible, a truce from the vehemence of the disorder. There may be particular circumstances in some agues that must require a variation of practice, but this is the case in every disease, and holds good against all general doctrine, and must, therefore, be left to the particular judgment of the physician. But if, after the ordinary treatment, or the long continuance of the disorder, the patients should grow languid and weak, and a general hydropic diathesis take place, we must have recourse to a different method of cure.

I now suppose the paroxysms have ceased, or return at very irregular periods, and also that they are incomplete in their several stages, while, at the same time, the patient grows extremely weak and bloated. In this state I have found it attended with an aggravation of all the symptoms to persevere in giving bark, and I am almost sure the miserable condition above described is generally brought on by the premature use of this medicine, or the improper exhibition of some other astringent. I must, however, observe, that there has been a particular obstinacy in the agues I have met with this season, as those of last year and the year before readily yielded to the common treatment. From the yellow suffusion in the countenance and skin in the *irregular intermittent*, we may naturally infer that a cachectic state has taken place; and from the foregoing connexion that was observed to obtain between the biliary secretion and the return of the paroxysms, we may suppose the liver the viscus chiefly engaged; add to this, that it is not uncommon to see evident enlargement of the right hypochondrium, and often the spleen becomes diseased, though, in my opinion, not so frequently in this country.

About a year ago a poor woman applied to me to be received into Steevens's Hospital. She had an enlargement of the liver and spleen, attended with all the marks of cachexy brought on from an ague improperly stopped by the too early administration of the bark. I gave her two grains of calomel every night, and purged her twice a week. When she had continued in this course for about ten days, she had a regular paroxysm of the ague, which had ceased to return for upwards of a month. After three or four irregular fits the type of the intermittent returned to its original form, a tertian, and she was vomited at the accession of every paroxysm, still continuing the use of the calomel. In the course of a fortnight or three weeks after this time the fits ceased altogether, becoming slighter and slighter, and the visceral congestions gave way gradu-

ally, so that she was dismissed, cured, in about two months from the time of her admission, without ever giving her any bark.

The success in this case induced me to think mercury possibly might be exhibited with advantage in the irregular state of ague described above, even where there was no mark of the viscera being engaged. But no cases occurring last year after this, I waited until this season, when I am certain I have had upwards of fifty patients in which I have given calomel with success.

In the course of this spring I had frequent opportunities of conversing with gentlemen returned from the East Indies, where I knew intermittents were apt to leave grievous complaints in the liver. I found from them that nothing was more common than a mercurial course for an obstinate ague, even where the liver did not show itself diseased. But, as this information did not come from medical authority, I still thought it equivocal. About this time I happened to see Mons. Senac's very ingenious treatise, *De Recondita Februm Intermittentium*, in which are many excellent observations. Among many remedies I found there had been a quack medicine sold at Paris which cured intermittents, and was afterwards discovered to be calomel and stibiated tartar, but he passes it over without recommending it.

Since this I have read a very pleasant, and, in many respects, a learned work, by Ramazzeni, on the diseases of tradesmen, in which he quotes a case, from a very old author, of a young man being very readily cured of an obstinate quartan, by salivation, as he was suspicious of having contracted the venereal disease, which was the cause of his being put under a mercurial course. He makes no farther use of this quotation than that possibly chemists and physicians might strike out a method of exhibiting mercury with advantage in fevers.

Weighing, therefore, all these matters, and being fully persuaded from experience that bark made the state of the ague above described infinitely worse, constantly increasing the hydropic tendency, I thought it fairly advisible to try how far calomel and stibiated tartar would answer; and that I might ascertain its sensible effects I have exhibited this combination of mercury and antimony in upwards of fifty cases. In three out of four of all the cases in which I have given this medicine, the paroxysms returned to their original type, generally speaking in about a fortnight, and sometimes in three weeks, unless in such cases as where a ptyalism has come on, in which instances I have seldom observed the recurrence of the ague; but I do not think they recover either so soon, or so certainly, as where the paroxysms re-assume their wonted periodical movements.

When matters are in this state I give antimonial vomits at the accession of the fit, and still continue the use of the calomel and stibiated tartar, until the anasarcous appearance has been removed, and that the patients have been sufficiently purged. For though

the general sensible operation of this medicine is by stool, it flies instantly to the salivary glands in some cases, and in others it produces no increased evacuation, in both which states it is necessary to purge the patient pretty smartly twice a week. The dose I have in general prescribed of this medicine is two grains of calomel and one of stibiated tartar [tartarized antimony], every night; and, as I observed already, its general operation is to occasion a revival of the paroxysms, which are to be treated as in an original ague; nor do I begin to exhibit the bark until six or seven fits have passed, and that the hydropic appearance is removed.

In most cases the intermittent ceases in six or seven paroxysms, and I seldom give the bark in this species of ague but as a general tonic medicine, after the febrile attacks have been entirely removed. Whenever ptyalism prevails much, I think I have seen it attended with bad consequences to administer the bark, though I have found wine extremely serviceable, a proof that their operation is not the same. The general dose of the medicine I before mentioned was two grains of calomel and one of stibiated tartar, taken every night, and the desirable end is a revival of the paroxysms, which, if properly managed, are nature's remedies to remove the disease: "Uti corpus suis viribus alimenta coquit sibique assimilat, ita non minus facultatem nactum est, morbosa et quæ præter naturam sunt subigendi atque contemperandi. Hinc ille tam salubris πεπασμὸς, quo conditiones noxiæ eorum quæ morbum intentant, aut faciunt, ita mitescunt, ut vel innocuæ fiant vel ad commodam ejectionem expeditæ. Quicquid in crisibus salutare est hac potissimum virtute nititur."

The whole, then, of my observations, may be reduced to this, viz.:—1st. That in the treatment of intermittents we should not attempt to stop the paroxysms until the patient has undergone plentiful evacuation by vomiting and purging. The contrary has, I fear, of late been too much the prevalent doctrine and practice. 2nd. That where the paroxysms have ceased, and a cachectic habit succeeded, it ought to be our endeavour again to excite and renew the fits; and where these have taken place, they are not to be disturbed by too premature an exhibition of the bark. 3rd. We should always have in view the natural propensity of intermittents to recur, the consequence of which will be a perseverance in medical discipline, though the paroxysms have ceased. Hence Celsus, "diem et horam diu meminisse oportet." But to you, gentlemen, I leave these remarks, to be confirmed or rejected by your observation. "Opinionum commenta delet dies natura judicia confirmat."

Dublin, 17th July, 1782.

Upon the Mechanical Treatment of Sterility. By Henry Oldham, M. D.

[From the Guy's Hospital Reports for October, 1849.]

"There have been three plans of treatment of a mechanical kind, for the cure of dysmenorrhœa and sterility, recommended and prac-

tised; and it is impossible for any one in practice in this city [London] as an obstetrician, and who reads the weekly and monthly journals, to be blind to the fact, that these means have of late been unsparingly and boldly employed. They consist, first, of the dilitation by metallic bougies or sponge tents, or by section of the os uteri internum and externum; secondly, of the removal of the front or back displacement of the womb by Dr. Simpson's uterine stem supporter; and, thirdly, by probing the Fallopian tubes. It is impossible for me to omit the notice of these expedients; although, if the womb be ascertained to be undersized, they would, I should hope, be abandoned in reference to it. No cutting, or dilating, or supporting, or probing, can make a small womb larger; and the amount of uterine stimulus which they would excite would be considered far too unimportant to justify their use. I know, however, that the characters of the reduced womb (if I may so call it) are not always appreciated in their entirety; and a source of error may arise from mistaking the natural and proportionate smallness of its orifice for a contraction to be removed mechanically. The anteversion I have noticed would by some be regarded as an efficient cause of sterility and dysmenorrhœa, and the uterine supporter be applied; while I suppose that Dr. Tyler Smith, if one or both these plans had been tried and failed, would *par voie d'exclusion*, consider it as coming within the undefined limits of tubal catheterism. The few remarks, however, which I shall make upon this subject, must be supposed to apply to the mechanical cure of sterility and dysmenorrhœa generally, without any strict application to these disorders as connected with the undeveloped womb.

"There are few cases which come before an obstetric practitioner which are so full of perplexity as those of sterility, especially where it is limited to those cases where the os and cervix, and body of the uterus, are free from any recognisable disease. Recent researches have afforded most valuable information on the composition of the male and female generative elements, and the physiology of generation; but our knowledge of the various causes by which impregnation is intercepted or prevented is very limited. One of these, no doubt, is any such partial or complete occlusion of the sexual canals as to prevent the transmission of the semen. Others are to be found in imperfectly developed ova within a shrunken ovary, or some defect in the semen, or a want of congruity between the two elements. These are subtle and concealed causes, difficult, and, with our present knowledge, almost impossible to detect, but of infinitely greater importance in their relation to primary sterility than the mechanical obstacles which have of late so exclusively engaged attention. It appears to me that the cases which justify the use of mechanical treatment require the greatest discrimination, not only on account of the facility with which they may be confounded with perfectly natural conditions, but also because these operations are not without danger. There is scarcely any amount of danger or pain that women will not go through to obtain

the prospect of becoming mothers. They are notoriously credulous as to success, and are the ready, and often the costly victims of empiricism; and I would venture to say that obstetricians ought to be nicely scrupulous in encouraging a plan of treatment of a very doubtful efficacy, and dangerous to life. I cannot imagine a position more overwhelmingly distressing to any right-minded man than to have been the means of destroying the life of a woman in the endeavour to remove sterility. And yet I am sure that, in these operations, a hazard is run quite disproportioned to the amount of good accomplished; and I shall recount two fatal cases which have come to my knowledge: and I cannot but infer that others of a similar kind have occurred, but have not been recorded side by side with those of a more fortunate issue.

"I feel great confidence in saying that the true congenital stricture of the os uteri, externum or internum, or of the Fallopian tubes, sufficient to prevent impregnation, is *very rarely* to be met with; and yet nothing is more easy, with the idea of a mechanical impediment in the mind, than to be self-persuaded into the belief that the natural orifice is too small. It is quite impossible to fix a standard size for the inlet to the womb. It has often happened to me to feel the virgin os uteri extremely small, and yet pregnancy to take place. The sound, too, is a very insecure guide to the measurement of the os internum; and I think it is a most reprehensible practice to allow a neuralgic dysmenorrhœa, whose seat I believe is generally in the ovary, to be the indication for this meddlesome practice. The only cases, in my opinion, in which a mechanical dysmenorrhœa with sterility can be said to exist, are those in which the tissue of the cervix is large and firm, and the os uteri is diminutive in comparison with the size of this body: a small, almost imperceptible, round aperture, perforating a bulky cervix. When the tissue of the cervix is not so condensed, but has its normal yielding feel, I doubt altogether the propriety of regarding even a very small os uteri as a strictured one. I have myself successfully treated by dilatation some cases of the kind above cited, but they are *very few*, compared with the large number which come under my care.

"I. I am indebted to my friend Dr. Golding Bird for the following instructive case. On April 7th, 1849, I received from him the uterus and appendages of a lady who had died from peritonitis, excited by attempts to cure sterility by mechanical dilatation, whose history, as furnished to me by Dr. Bird, is as follows, and with whose concurrence I publish it:—

"'A lady of dark complexion, aged 36; married several years, and never pregnant; resided in Jamaica. From youth she suffered intense dysmenorrhœa, and always had pains during sexual intercourse. She was nervous, hysterical, and excitable to the last degree, and was supposed to have suffered from every possible form of inflammation; these attacks obviously being neuralgic, so common in hysterical women. In June last, by the advice of her physician in Jamaica, she came to London for the express purpose of

having the os uteri dilated, which had already been attempted by wax dilators. The obstetric physician who was consulted in London coincided in this opinion, and thought the sterility and dysmenorrhœa depended on a stricture of the os uteri. He divided the os uteri with a cutting instrument, and introduced silver dilators. This produced horrible suffering; and although at first she fancied the pains of menstruation were rather better, they soon became as bad as ever, and she did not experience the slightest relief. She left off the treatment for a time, but was soon again inclined to resume it; and silver canulæ were passed into the os, and left there. Again she suffered frightfully. On Saturday, March 31st, a gentleman, the assistant of the physician, passed in another tube, but the distress was intolerable; and sickness and shivering coming on, she urgently begged her sister to try and remove it, which she succeeded in doing. Getting worse, a neighbouring surgeon was summoned, and he found her labouring under what he regarded as peritonitis masked by hysteria. She had scarcely any fever, collapse coming on almost immediately, and she continued sinking until Tuesday, when I (Dr. G. Bird) was summoned to her. I found her at her sister's residence at T—— Park, presenting almost the collapse of cholera: pulse 200, and a mere thread; distended abdomen; vomiting of black fluid; intense irritability. All treatment was useless, and she soon sunk. On examining the body, and raising the omentum, no appearance of disease of any kind was found above a line connecting the anterior superior spinous processes of the ilia. Below this line there was intense peritonitis: the convolutions of the intestines covered with butter-like lymph, and the pelvis filled with pus-like fluid: the right ovary and broad ligament covered with the same butter-like lymph, but so feebly adherent that it washed away by dipping it in water: the cavity of the uterus was filled with bloody mucus. There was no other disease.'

"The uterus and appendages were examined by Dr. Oldham.

"The uterus had been opened by a single oblique division of the anterior wall, directed from the cervix to the left angle of the womb. The uterus was larger than usual for the virgin: it was rounded on its anterior surface, and a bulging convexity of the posterior wall, which, with the general softness of the tissue, showed it to have been the seat of recent engorgement.

"The blood-vessels over the entire surface of the uterus and appendages were injected with blood, especially the fimbriated extremity of the tubes, the ovaries, the broad and round ligaments. On the anterior surface of the body of the uterus were two small projecting fibrous tumours, the size of a large and small pea: the serous investment of them was highly vascular, the blood-vessels rising over them just like the calyx of the ovarian ovum of the bird. There was a similar more flattened growth in the posterior wall.

"The divided surface of the anterior wall showed its proper structure to be much enlarged (it measured in the body eight lines);

the muscular structure was soft, and the veins large: a probe easily ran through them. The length of the united cavities was two inches and ten lines, the canal of the cervix being one inch five lines. The mucous membrane of the cavity of the body was soft, slightly raised, and of a vermilion hue. Agitation in the water was sufficient to loosen and separate it.

"At the os uteri internum there was a zone of highly injected blood-vessels, broken only at one point: the circumference of this aperture was eight lines. The os externum had a clean, smooth edge, without any break or mark of division: its circumference measured one inch one line. The cervix had its characteristic markings, and the glands were empty of mucus. On the right side of the divided cervix, which would have formed the front wall, the ribbings were stretched upwards, enlarging the mesh-like appearance; and towards the os internum some were lacerated transversely, and from this to the os externum the structure was more ragged than usual.

"*The Right Tube.*—The extremity of this tube was almost entirely closed as a congenital formation, the aperture being very small. When opened, the fimbriated end showed its characteristic rich folds of mucous membrane, which were much injected, and were covered with bloody mucus. The remaining two-thirds of the tube was apparently healthy, not vascular, and pervious throughout.

"The right ovary, which was almost covered with lymph, soft and large. There was a cyst large enough to hold a small nut on the uterine end of the ovary. The stroma was gorged with blood. There was only one puckered Graafian follicle: the surface of the ovary was thick and corrugated.

"The left ovary was irregular in its shape, a projecting mammillary portion coming out from its outer end. This, on being cut into, was hard and vascular, like the commencement of malignant disease: the ovarian tunic was thick and wrinkled; the stroma, vascular: a few remains of Graafian vesicles, with puckered tunics, and some clots of different colours, black and brownish.

"The left tube vascular at its fimbriæ, healthy in its mucous membrane, and its canal pervious throughout. This tube passed into the uterus more directly than its fellow, which was more curved. The veins healthy; arteries healthy; the right round ligament large and vascular; vagina healthy.

"It is unnecessary to comment at any length upon this case. It affords a most instructive example of the dangerous effects of dilatation, even in experienced hands, and the great caution with which it should be undertaken. It is important, too, as showing the difficulty of detecting the cause of sterility. I am sure that there was no kind of morbid contraction in this case, and that the os and cervix uteri, which were alone treated, had nothing whatever to do with the dysmenorrhœa or sterility. Both of these, no doubt, were dependent on the atrophy of the ovary; and the congenital obliteration of the end of the right tube would have been

sufficient to exclude the corresponding ovary from any share in the function of reproduction.

" II. Another presumed cause of sterility and dysmenorrhœa is, any deviation in the position of the uterus, and hence an indication for the cure of these disorders is to replace this organ, and hold it in its proper axis in the pelvis, by means of Dr. Simpson's uterine supporter. Dr. Rigby and others have related cases of this kind. It is not necessary for me to reiterate the objections which I urged in the last Number of the Reports upon this subject, but I cannot avoid relating the following case, which more than confirms my opinion of the dangers which may arise from this supporter. I am indebted to Mr. Bransby Cooper for this case, which, like the preceding one, ended fatally; and which he has given me his permission to publish.

" A young married lady, of great personal attractions, was attended by Mr. Cooper, for a very painful fissure in the anus, which he divided and speedily cured. She then spoke to him of what had been to her a very distressing social trouble, namely, her sterility, which she associated with a perfect indifference to sexual intercourse. Mr. Cooper examined the sexual organs; but as he did not discover any defect which could be remedied by surgery, he referred her to a physician-accoucheur. This gentleman detected the uterus in a retroverted state, which he looked upon as the probable cause of the sterility. For the cure of this displacement, he introduced a uterine stem supporter, which set up peritonitis, of which she died in three days.

" It is much to be lamented that the warning which such a case as this imperatively suggests should not have been published by the obstetric physician in whose practice it occurred. My own opinion is, that mere displacement forwards or backwards, if the uterus be not diseased, does not commonly cause sterility; and I cannot but characterize the practice of fixing the womb in a definite position by means of a stem supporter, as rash and hazardous, causing severe irritation and pain, and even death, to the patient, with, at the best, a very questionable amount of ultimate good. The anteversion or retroversion of a small uterus, without other complications, does not, in my experience, occasion any great distress; and it is far better to leave it alone, and improve its tissue with the rest of the organs of the body, than to prop it up for a time under the feeble pretence of curing it.

" III. Dr. T. Smith's adventure of catheterizing the Fallopian tubes I know of only from his papers. I have the instrument by me, but at present I have no intention of using it."

MEMOIR OF BRYAN HIGGINS, M. D.,

AND OF

WILLIAM HIGGINS,

PROFESSOR OF CHEMISTRY TO THE ROYAL DUBLIN SOCIETY;

WITH A SHORT NOTICE OF IRISH CHEMISTS AND THE STATE OF CHEMISTRY IN IRELAND BEFORE THE YEAR 1800.

By WILLIAM K. SULLIVAN,

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To many persons the present essay may appear useless;—they may ask, Why take any trouble about such unimportant memoirs as the mass of those which I am about to mention. What has Ireland added to chemistry? I answer, that the history of science is not made up of great discoveries alone; that the more obscure workers have a right to be remembered with honour, as the unhewn stone helps to make part of a building as well as the ornamental column. But, independently of its reference to the history of chemistry in general, such an essay as the present is of considerable value in a national point of view, in showing the circumstances which tend to advance or to retard knowledge, and hence the way to remove the causes unfavourable to progress. It is also a species of barometer for indicating the influence of governments, of political changes, and of sectarian dissensions, upon the activity of the human mind. In Ireland this is remarkably exemplified, as the reader will perceive who compares the dates of our political crises with those of the flourishing periods of chemistry. Hitherto our scientific men have, with the exception of a few medical ones, and one or two who were connected with literature or politics, been altogether neglected. We have, it is true, very little to boast of; but a glance at the few things which even I have been able to collect, will, I am sure, convince all that, though we cannot lay claim to many brilliant discoveries, it has not been from want of an earnest desire to keep pace with other countries, and that the record of many of our societies show that, under better circumstances, we might have held an honourable rank in science in Europe. I believe that no country has made so many struggles to advance in knowledge, under similar disadvantages, as Ireland has; and to those who know the history of this country, it is unnecessary to say why these efforts were unsuccessful.

The task which I have had to perform was one by no means easy, owing to the difficulty of distinguishing who were Irishmen and who were not. In the case of men of great eminence this did not often occur, but even among such there have been too many examples of that rather Irish vice of discarding their country,—a crime which a Scotchman looks upon with horror, although he may be the warm-

est advocate of England; and he is right, for whatever opinion we may entertain individually, as to the means of advancing the country, there is one thing which all people may be justly proud of, a separate literary and scientific existence. I have considered as Irishmen (a very few excepted), only such as spent a portion of their mature age in the country, and of course I have made no distinction between those born in Ireland and those who, though born in another country, may have brought their talents amongst us. It is probable that I have not been able to distinguish all the names which should be included, but I may be permitted to hope that such deficiencies will be, at some future time, filled up, according as people learn the value of such records.

That the early Irish had some knowledge of alchemy there can be no doubt, for we find among the works attributed to the celebrated John Duns Scotus, several essays on that subject, which must have introduced the dreams of these visionaries into Ireland(*a*). And though our records are silent on the subject, we find, so early as the middle of the sixteenth century, several Irishmen having considerable reputation as adepts in the mystic science. The best known of these was Edward Kelly. His real name was Talbot, and having, when in Ireland, forged several public documents, he fled to Wales, and from thence to London, where he became acquainted with the well-known Dr. Dee, whose reputation he helped, and with whom he went to Prague, to the court of Rodolph II., who was a great patron of alchemy. He there converted, it is said, several times, mercury into gold; and as this was the first real proof which the Emperor had of the truth of the hermetic art, he ennobled Kelly. But his prosperity soon came to an end, for the Emperor took a fancy to know the secret of the philosopher's stone himself, and in order to force it from Kelly he cast him into prison, in endeavouring to escape from which he broke his leg, and soon afterwards died(*b*). Another Irishman, of the name of Butler(*c*), enjoyed considerable reputation for his skill in metallurgy and in alchemy, which he is said to have acquired from the Arabs, by whom he was made prisoner and sold as a slave. He died in 1625. But it is only from the middle of the seventeenth century that we can date the commencement of the study of experimental science in Ireland. The continual state of warfare in which the country was always plunged

(*a*) Borellius, quoted in Gmelin's *Geschichte der Chemie*, gives the following of these tracts:—1. Dominus vobiscum; 2. Tractatus ad Album et Rubrum; 3. Tractatus ad Regem Angliæ; 4. Opus magnum, secundum Intentionum omnium Philosophorum verissimum; 5. De Veritate et Virtute Lapidis Philosophici.

(*b*) Two works were published by him, viz.: 1. *Tractatus Egregii de Lapide Philosophorum cum Theatro Astronomiæ Terrestris in Gratias Filiorum Hermetis in Lucem.* Editi a Io. Langio. Hamburg, 1676. 8vo. It was also translated into German. 2. *Fragmenta a Cambachio edita.* Geism., 1647. 12mo.

(*c*) Mentioned by Van Helmont, *Opera omnia apud Elzevir*, 1648, p. 582.

was much more fatal to the fine arts and to experimental science than to the study of theology or metaphysics. Add to this that the insular position of Ireland, at all times more or less unfavourable to the spread of ideas from without, completely shut us out from benefiting by discoveries made on the Continent, or from being affected by the great fermentation of mind which began to agitate Europe after the Crusades, and which had an extraordinary influence on the progress of all the natural sciences.

This state of things also prevented the growth of manufactures and the cultivation of the arts, the parents of experimental science, especially of chemistry. We have thus sustained an irreparable loss, for at present we have not only to create the manufactures which should have been the slow growth of years, but we have to induce, at the same time, a taste for experimental science, which would have, under favourable circumstances, accompanied this slow growth, had such taken place. The first man connected with Ireland who did anything of importance in chemistry appears to have been Bernard Connor, who was for some time physician to the King of Poland at the end of the seventeenth century(*a*). He is the first writer with whom I am acquainted who appears to have seen the importance of the application of chemistry to physiology and pathology; he even mentions the variation of the quantity of fibrine, as well as of the entire clot, in several diseases, and points out many other facts which, for that time, were very curious(*b*). Like most of our great men, however, he lived principally on the Continent, and consequently had very little influence in furthering a spirit of inquiry at home. A country gains very little intrinsically from the birth of a great man, unless he remains there, publishes his labours there, and, above all, communicates his ideas, his spirit, to those around him. The greatest blessing which can be bestowed on a country is the birth of a great man, and the greatest misfortune which it can sustain is the loss of one; and the latter has been the lot of our unfortunate country, her illustrious sons belonging more properly to the history of other countries than to our own. The great Robert Boyle is an example of this; though an Irishman by birth and connexions, the influence of his genius was shed on England for the greater part of his life. But happily he did not entirely forget his native land; a ray of his influence fell on two men whose names are indissolubly connected with the true interests of Ireland,—the two Molyneauxs,—through whom the first era of our modern progress commenced. In the year 1683, William Moly-

(*a*) A compendious Plan of the Body of Physic. Oxon., 1697. *Dissertationes Medico-Physicæ.* Oxon. 1695–8.

(*b*) At page 21 of vol. xix. of the Philosophical Transactions we find a paper by him under the title, "An Account of an extraordinary Human Skeleton whose Vertebræ of the Back, the Ribs, and the several Bones down to the Os Sacrum, were all firmly united into one solid Bone, without Jointing or Cartilage."

neaux founded the Dublin Philosophical Society, in which Boyle, then in the height of his fame, took a great interest, and principally through him the Royal Society offered to publish their proceedings, as they were not in a condition to do so themselves. I shall not enter into the history of this Society, as it has already been very fully given in the Editor's Preface prefixed to the first Number of the present series of this Journal, except in so far as it has reference to the subject of Chemistry, to which its members paid considerable attention. They appear to have had even a chemical laboratory, as is evident from a letter from William Molyneaux to his brother Thomas, then residing in Leyden. This laboratory was constructed under the direction of Dr. Mullen, and was situated in a place called Crow's Nest, probably the site of the present School of Medicine of the Apothecaries' Hall in Cecilia-street.

The papers read at the Society appear to have been quite equal to the generality of the memoirs read at the societies then in existence elsewhere; and though no brilliant memoir appears among them, there are a great many nice observations, which show that the men who wrote them had a considerable knowledge of the subjects treated of, and in many cases very comprehensive notions of the sciences. The persons who contributed the chemical papers were William Molyneaux, Lord Mountjoy, Sir William Petty, Richard Bulkely, Mr. Patterson, Dr. Houlaghan, Mr. King, Dr. (afterwards Sir Patrick) Dun, Dr. Silvius, and, above all, Dr. Allen Mullen, who appears to have been a very superior man. It is unnecessary to give a list of the papers read, as they have been already given in the Preface above alluded to(*a*).

This Society, which gave so much promise, and which would, no doubt, have added considerably to the domain of chemistry, had existed only during three or four years, when the civil war broke out, and put an end to all further improvement. It was, however, again revived in 1693, but only for a very short time. Another attempt was made to establish it in 1707, when several papers were read, of

(*a*) The labours of some of the members of this Society were not confined to the papers read before it. Thus we find that Dr. Edward Smith published, in 1695, an examination of a soapy earth from Smyrna, with the process there followed for making use of it as soap (Phil. Trans. vol. xix. p. 228), in addition to his essay on the petrifying nature of the water of Lough Neagh, which was read to the Society. Dr. Moulin or Mullin, besides the papers enumerated in the Editor's Preface, published the following:—"An Experiment of the Injection of Mercury into the Blood, and its ill Effects on the Lungs" (Phil. Trans. vol. xvii. p. 486); "Some Experiments on a black shining Sand brought from Virginia, supposed to contain Iron (Titanate of Iron)" (Phil. Trans. vol. xvii. p. 624); "Anatomical Observations on the Heads of Fowls" (Phil. Trans. vol. xvii. p. 711); "Analysis of a Water from near Canterbury" (Phil. Trans., vol. xxv. No. 312, 1706-7),—from this latter paper it would appear that he left Ireland and settled near Canterbury; and a paper on the Anatomy of the Elephant, Dublin, 1682, one of the earliest on record.

which two were chemical, namely, one by Thomas Molyneaux, on the Mines and Minerals within the kingdom of Ireland,—the first notice of the kind which appears to have been made,—and the other by William Molyneaux, on Mercurial Phosphorus. But the country was reduced to such a condition that science, though supported by the energy and patriotism of the Molyneauxs, could not obtain an asylum, even with a select few, in the capital, and the Society, after this second resuscitation, ceased to exist(*a*).

After the Philosophical Society had ceased to exist, a long interval occurred before attention was again directed to science. In the year 1731, however, the Royal Dublin Society was founded, and although chemistry did not occupy much of its attention at that time, the spirit of inquiry which led to its establishment extended itself also to chemistry, for we find that in the very same year Dr. Thomas Madden showed the poisonous and acid nature of common laurel water(*b*); and in the year 1739 Dr. John Rutty wrote a dissertation on the same subject. About the year 1737 Boyle Godfrey, whose epitaph was given in the twelfth Number of this Journal, published an excellent paper on the watery extract of opium, in which he accurately pointed out many of its properties(*c*); he also wrote an essay on artificial bole, with its use in making transparent slates, and an analysis of a water from near West Ashton(*d*). It is remarkable that Trinity College should have given, even up to a very late period, so little encouragement to experimental science; for we find that nearly all the papers on chemistry, with few exceptions, written during the period of which we write, were not only published in England or elsewhere,—a matter not very surprising, considering the state of the country,—but were the productions of men who were educated either at Edinburgh or Paris. Boyle Godfrey is an example of the former, and Andrew Cantwell of the latter. Cantwell published very many chemical essays, among others, one, in connexion with a Frenchman named Hazon, on the subject which occupied so much attention at that time, the cure of the stone by alkalies, under the title of *Experimenta in Calculi Acetate et Temperamenta Ægrotantis Remedium alcalino Saponaceum Anglicum*, Paris, 1742, 4to. He wrote many other papers, principally

(*a*) An idea may be formed of the state of the country at the end of the seventeenth century by a curious passage in the *Cours de Chimie* of Lemery, relative to the *usnea barbata*, which was then much employed as a medicine in epileptic fits. It was considered doubly valuable if it had grown on a human skull, especially of those executed; and as that from Ireland possessed this great merit, it became an *article of commerce!* for the reason which Lemery thus states, "Car en ce pays-la on laisse les hommes qu'on a pendus, attachés à des poteaux dans la campagne, jusqu'à ce qu'ils tombent par piéces; os. pendant ce temps-là la chair et les membranes de la tête s'etant consumées, cette mousse naît sur le Crâne."—*Cours de Chimie*, p. 856,—1675.

(*b*) Phil. Trans. vol. xli. p. 2, No. 461, 1740.

(*c*) Phil. Trans. vol. xxxvii. p. 84.

(*d*) Medical Essays and Observations by a Society of Physicians in Edinburgh, vol. v. p. 2.

on medical subjects, five of which are to be found in the Philosophical Transactions. About the same time Bond and Henry described accurately the petrifying qualities of Lough Neagh(*a*), a subject which appears to have excited great attention at that time, judging from the number of papers published on it. Even the celebrated Berkeley has left some remarks on it(*b*). Bond and Henry appear to have always taken up the same subject, for we find two papers by them, giving an account of some experiments on the copper springs of Wicklow, in the Philosophical Transactions(*c*).

The spirit of inquiry rapidly increased, and was finally concentrated in the Medico-Philosophical Society, which was founded in 1756, and which was the most useful and active society which existed in Ireland during the last century(*d*). Its papers were generally medical, but it also paid considerable attention to physics and chemistry. Among its most active chemical members were Dr. Macbride, Mr. Bride, an apothecary, who appears to have done a great deal for pharmacy in Ireland, Dr. Barry, Dr. Charles Smith, Dr. Purcell, Dr. Rutty, and some others. The man of most ability among them was, unquestionably, Dr. Macbride. Dr. Rutty was the most active and persevering; he had always a paper of some kind for each meeting, but, with few exceptions, they are scarcely worth mentioning, and are very deficient in philosophical ideas.

As very little is known of this Society, except the notices which Mr. Wilde has from time to time brought forward in this Journal, though it occupies a very high place in the history of scientific progress in Ireland, particularly in that of medicine, for it may be said to be the foundation of our Irish School of Medicine, it may not be uninteresting to give a short account of the chemical papers which were read before it, in order to show how rapid was the progress then taking place in Ireland, as exhibited by the spirit of scientific research, which, in a few years afterwards, led to the establishment of an Irish Parliament.

Shortly after its foundation, Dr. Charles Smith, well known by his Histories of Cork and Waterford, works which, by the way, were undertaken at the suggestion of the Society, and the publication of which was assisted by its funds, read a preliminary discourse in which he pointed out the value of such a Society, the necessity for the establishment of one in Ireland, and on the then state of affairs in Ireland, from which I shall give an extract, illustrative of the latter point, and which places the men who founded such a body, under such difficulties, in a very favourable light.

"Ireland has made but few attempts to add one to those illustrious bodies which have been long since founded in every civilized

(*a*) Phil. Trans., vol. xlvii.; vol. xlviii. p. 1, 181; No. 84, p. 500.
(*b*) Phil Trans., vol. xliv. p. 305.
(*c*) Bond, vol. xlviii. p. 181; and Henry, vol. xlvii. p. 500.
(*d*) See the Editor's account of this Society and its Memoirs in his Preface to the present Series of this Journal, vol. i. p. xxviii. See also Dr. Aquilla Smith's communications on the history of Irish medicine, referred to therein.

state of Europe; for even the Russians, a people but the other day reclaimed, by a great prince, from barbarism, have stepped before us, not only in forming the Royal Academy of St. Petersburgh, but also in publishing Philosophical Transactions which might do honour to any other more civilized nation.

"It might be hoped that the present institution, how crude soever it may now appear in its infant state, might serve as a spark to kindle such design in this kingdom, when a more favourable opportunity presents itself. But, to speak the truth, none of the members are so sanguine as to flatter themselves with ever seeing so agreeable a prospect. The present times are remarkably distinguished for an invidious turn of mind in most persons capable of prosecuting such an undertaking, and, sorry are we to add, by an utter contempt and disregard of every one whose industry or abilities might appear superior to their own. These men, by an abject flattery, a studious devotion to the low arts of party or faction, and many other mean ways of distinguishing themselves, endeavour to engross the confidence and protection of men in power, who are themselves entirely occupied, and have their time taken up, with every method of aggrandizing their families and supporting luxury (now grown to an immoderate height), even at the expense of all that is dear to the well-wisher of his country. Much more might be said on this head consistent with integrity, but this slight sketch of things must suffice; it is neither visionary nor false, and consequently the hopes of either capable or ingenuous persons who might mutually agree to support each other in carrying on a scheme of this kind, nor a sufficient number of people in power who might protect and encourage it is hardly at this time to be expected."

The hopes which were visionary to Dr. Smith at that time were, however, realized, but only for a moment, during the first years of enthusiasm and progress which followed the establishment of Irish independence. The spark, however, produced no flame; it was almost completely extinguished by the unheard-of corruption of the Irish Parliament.

During the year 1756, Dr. Rutty(*a*) read a few insignificant notices on mineral waters, but the first chemical paper really deserving of notice was read by him on August 3, 1757. In this paper he pointed out the difference between the varieties of tannin which produce green and black precipitates with protosulphate of iron. In November, 1757, Rutty also showed that alkaline salts attenuate the blood, that is, diminish the blood globules, and soften flesh.

During the year 1758, Dr. Smith read some ingenious observations on the cementing principle of sandstones; he, however, attributed this effect to the presence of vitriolic or sulphuric acid, carbonic acid not being then well known; and Dr. Rutty read a paper on some experiments made on woman's, cow's, goat's, ass's, and sheep's milk, which was afterwards published in full under the title of

(*a*) See Rutty's Memoir in the sixth Number of this Journal.

"Analysis of Milk, and the different Species thereof, 1762," in which he showed the coagulability of woman's milk by rennet, pointing out at the same time the comparatively small amount of caseine which it contains.

Several chemical papers were read during the year 1759; among others, one by Dr. Rutty on the bastard alkanet (*anchusa tinctoria*), in which he pointed out the solubility of its colouring matter in oils; Dr. Barry, on salts discovered in the urine of persons ill of fever, which he stated were like nitrate of potash, viz., hexagonal prisms with pyramidal bases, and some cubical like the marine salt; and three by Mr. Bride, on the adulterations of saltpetre; on the gum *rubr. gambiensis*,—a pharmaceutical paper quite equal to those published in other countries at that time; and on *mercurius dulcis*, or calomel, containing some excellent observations.

The papers read during the year 1760 were by no means equal in value to those previously mentioned; the best was one by Mr. Bride, entitled, "A Chemical Remark on the last Edition of Quincy's Dispensatory," in which he, as well as Dr. C. Smith, pointed out the difference between the action of potash and ammonia on salts of mercury.

In 1761, Dr. W. Redmond read some chemical and metallurgical observations and experiments upon antimony, containing some excellent remarks on the action of alkalies on the sulphurets of antimony. This essay was published under the title, "The Principles and Constituents of Antimony: London, 1762. 8vo."

In 1762, Dr. Rutty communicated the result of an analysis of a liquid taken from a dropsical patient, in which he showed the alkalinity of the liquid, and the precipitation of albumen by heat, and by corrosive sublimate, and several other tests.

Dr. Macbride(*a*) read an account of some experiments showing the power of different acids to sweeten putrefied animal substance; as also the antiseptic quality of the vapour set free from substances during the first stage of fermentation.

In some subsequent papers Dr. Macbride showed that lime is precipitated from lime-water, by the gas set free during fermentation and putrefaction; and this observation he applied to account for putrefaction. He also showed the nature of the reaction which occurs when carbonate of potash is added to solutions containing lime. In these papers he clearly showed the existence of fixed air or carbonic acid in limestone, but accounts for the solubility of the latter in water by its being deprived of this fixed air and converted into the same condition in which it exists in lime-water; this was also the opinion of Cavendish, showing that they were unacquainted with the existence of bicarbonates.

In 1764, Dr. Macbride exhibited some drachms of a volatile alkaline spirit, obtained by distillation at a very gentle heat from putrid blood. This substance he showed to possess all the proper-

(*a*) See his Memoir in No. V. of this Journal.

ties of ammonia, an opinion quite contrary to that then generally received. He also obtained it from bile, meat, &c.

Dr. Macbride published in the same year, in a collected form, the results of all the experiments which he had communicated to the Society, under the title "Experimental Essays on the following Subjects:—1. On the Fermentation of alimentary Mixtures. 2. On the Nature and Properties of Fixed Air. 3. On the respective Powers and Manners of Acting of the different Kinds of Antiseptics. 4. On the Scurvy; with a Proposal for trying new Methods to prevent or cure the same at Sea. 5. On the dissolvent Power of Quicklime." It was translated into German by Conrad Rahn, under the title of "Durch Erfahrungen erläuterte Versuche, &c.," Zürich, 1766; and into French by M. Abbadie, under that of "Essais d'Expériences sur la Fermentation des Mélanges alimentaires sur la Nature et les Propriétés de l'Air fixe, &c.," Paris, 1766.

This was, for its time, a very remarkable work, and has gained for its author a respectable place in the history of chemistry. His principal merit consisted in perceiving the value of Dr. Black's discovery of fixed air or carbonic acid, and fixing the attention of physicians and chemists on the important part which it plays in many processes of animal and vegetable life. Thus he considers that all bodies in nature derive their force, their consistence, and the cohesion of their parts, from the fixed air which they contain, and when deprived of it by any means, the molecules no longer adhere together; hence results, according to him, putrefaction in those bodies which are capable of undergoing it, and those which are not fall to powder, as lime does when dissolved in water. He examined very carefully the production of carbonic acid during the process of the fermentation and putrefaction of animal and vegetable substances, the latter bodies containing, according to him, more of it than the former. According to his views, these two phenomena depend, in a great measure, on the disengagement of this gas, and consequently cease when it ceases to be evolved. In the same manner he accounted for the action of carbonic acid in arresting putrefaction, which he was the first to observe, by giving back the fixed air which the body had lost. On this view was founded his treatment of scorbutus, which disease he defined to be "putrid disease, from the want of this bond and cement of bodies," by liquids such as beer, that contain fixed air. He explained the action of caustic lime in furthering putrefaction by its affinity for fixed air, which it drew from the body with which it was in contact. He also pointed out the existence of carbonic acid in fresh-drawn blood, and also in small quantity in air which he showed to be entirely different from it.

We have just mentioned his valuable observation with regard to the existence of volatile alkali, or ammonia, in the products of putrefaction, and in the products of the distillation, *per se*, of various animal substances.

Dr. Macbride did not confine himself to scientific chemistry, for

in the Philosophical Transactions we find a paper of his on an improved method of tanning leather (vol. lxviii. part i., p. 111, 1778), being, we regret to say, one of the very few practical papers by Irishmen which we have been able to find. This process appears to have been practised in Dublin, and other parts of Ireland, under his direction, and was originally communicated to the Dublin Society. It consisted in extracting the bark by means of lime-water, the skin having been prepared in the usual manner. He also recommended sulphuric acid instead of the sour liquid obtained from animal and vegetable matter, for opening the pores of hides for sole leather, and thickening, or *raising* them, as the tanners say.

In April, 1765, Dr. Rutty described the urine of a man having diabetes, in which he mentions the extract as having the smell of burned sugar, and a perfectly sweet taste. It does not, however, appear that he considered it as such. I am not aware of the fact having been noticed earlier.

Between this period and October, 1767, there were several papers read, but none of them of any great importance, being principally notices of things already observed. In that month Mr. Cleghorn communicated, on the part of Dr. Black, a method of preparing the *cuprum ammoniacali* (*cuprum ammoniatum* of the Pharmacopœia), which was found useful at that time in cases of epilepsy. I mention this paper not so much on account of its intrinsic merits, but rather to show that although Dr. Black had ceased to be an Irish reident, he, like Robert Boyle, still continued to cast a ray of his influence on his native land, for that he was born in Ireland there can now be no doubt. But, as we have already said, it is not the country of his birth which can be proud of a great man, but the country which enjoys the influence of his genius; Scotland is therefore fully entitled to claim Black as one of her sons, but we may still be proud that he was not entirely lost to us.

From this period until 1766, very few chemical facts were communicated to the Society. Dr. Rutty, however, continued to make observations on his favourite subject, mineral waters, and on the phlogiston theory, which then occupied the attention of the whole of Europe. We must not, however, conclude that the Society had diminished in activity, for its journals for these years are full of valuable medical observations. In January, 1766, Mr. Bride read a paper "On the Fermentation of Corn, and the Distillation of Corn Spirits," which was truly admirable for the time. In this paper he very properly points out that the spirit is derived from the sugar, and that the sugar which is perceptible in the commencement of the year in the juices of plants, disappears in the autumn, and is replaced by farina, and that the process of malting consists in an incipient vegetation in which this sugar is formed. He states that there can be no doubt but that by malting corn a crystallizable sugar may be obtained. He also alludes to the fact that malted corn

is capable of converting raw corn into the same state as itself. In the same year, Dr. Purcell read a very excellent paper on a limestone gravel, in which he pointed out the mode which he adopted for its analysis. This is one of the best essays on this subject, of that period, which we have ever seen; indeed some of the observations contained in it are far in advance of many articles of more pretension which appear in the journals of the present day.

Nothing of any moment, connected with chemistry, was communicated to the Society until November, 1783, when Mr. Bride read a very ingenious paper on Manures; and in January, 1784, Dr. Perceval showed that water boiled at a higher temperature in glass than in metal vessels. The last meeting of the Society, of which we have any record, took place on the 7th of October, 1784.

This Society seems to have been very well managed, and to have been quite adapted to the wants of the time. Specimens of interesting plants, of substances used in medicine, &c., were usually exhibited at its meetings; the latter, particularly, by Mr. Bride. The members paid some attention to the microscope, and experiments were often exhibited at the meetings. The following entry is recorded in the memoirs: "January 18, 1759.—This evening the Society met at the Rev. Mr. Jones's, who exhibited some magnetical experiments before them, and also a view of the crystallization of the solution of sal-ammoniac and other salts in the microscope." All the new books published on the subjects which engaged the attention of the Society were noticed, and sometimes extracts from them were read to the Society.

Up to the time of his death, Dr. Rutty regularly presented to the Society remarks on the weather, with tables of the variation of the thermometer and of the fall of rain, which, although not very accurate, are still very valuable, and show that the importance of meteorology was fully appreciated by the Society.

The Medico-Philosophical Society did not, however, include among its members all the workers in science then in Ireland. Among the Irishmen not members of the Society, who turned their attention to chemistry, we may mention Thomas Lane, a native of the south of Ireland, who practised for some time in London, but also resided in Ireland. He showed, in the year 1769, the solubility of iron in water impregnated with carbonic acid(*a*). He also invented the form of electrometer known as Lane's Discharger(*b*). We also find him writing on the great subject of that quarter of a century, the stone, for which he proposed as a solvent lime-water(*c*); and in 1791 he published a chemical examination of urine(*d*). Owing to the want of the means of obtaining education at home during the whole of the eighteenth century, the Roman Catholic por-

(*a*) Phil. Trans., vol. lix. p. 1; No. 30, p. 216.
(*b*) Phil. Trans., vol. lvii. p. 451.
(*c*) Medical Transactions by the College of Physicians in London, vol. i., London, 1768.
(*d*) Phil. Trans., vol. lxxxi. p. 2.

tion of the young men of talent were compelled to seek it in foreign universities; but unfortunately this class, which had thus such opportunities of acquiring the continental methods of investigation, were compelled on their return home to renounce science, and to attend to the engrossing pursuits of the profession to acquire a knowledge of which they had gone abroad. At that time, also, there was no field open to them of a scientific character; and from the paucity of physicians belonging to their religious denomination at that time, and the poverty of the people, the whole of their time was completely occupied. As to the Roman Catholic priests of that period, they were unfortunately all educated in foreign colleges where the whole course of study consisted in metaphysics, &c., and where experimental science was little attended to; still we find a few of that class who deserve to be mentioned, if it was only for their rarity. James O'Reilly, for some time physician to one of the Austrian princes, for instance, published an excellent analysis, for the time, of the mineral water of Steckniz, in the year 1766(*a*).

Dr. Rutty, in addition to the papers which he read to the Medico-philosophical Society, communicated five papers to the Royal Society, viz.:—1. "Account of the Copper Springs lately discovered in Pennsylvania"(*b*). 2. "On Sulphur in Water"(*c*). 3. "Of the Vitriolic Waters of Amlwch in the Isle of Anglesey"(*d*). 4. "Occasional Remarks on the Hartfell Spas, and their Comparison with other Waters of the same Class"(*e*); and 5. "On the Poison of Laurel Water," already mentioned. He also published a general work on mineral waters, entitled, "A methodical Synopsis of Mineral Waters, comprehending the most celebrated medicinal Waters, both cold and hot, of Great Britain, Ireland, France, Germany, and Italy, and several other Parts of the World." London, 1757-8 to 1761. 4to. This work produced a controversy between him and the celebrated Lucas, who published an answer to Rutty, entitled, "An Analysis of Dr. Rutty's Methodical Synopsis of Mineral Waters." London, 1758. 8vo. The subject of mineral waters was as much a favourite with Lucas as with Rutty, for previous to the controversy he had published a work on the subject, under the title of "An Essay on Waters, in three Parts, treating of simple Waters, of cold medicated Waters, of natural Baths,"—London, vols. i.–iii. 1756 to 1758,—which was translated into German by I. E. Zeiher, under the title of "*Versuch von Wassern, Altenburg;*" vol. i. 1767; vol. ii. 1768; vol. iii. 1769. 8vo. A work very far superior to anything of Rutty's. The controversy was principally on the subject of the presence of sulphur in water. According to Rutty, it was to be found every-

(*a*) *Tractatus de Ortu ac indole contentis, viribus Medicis ac debito usu Aquarum Mineralium.* Stecknizensium Ponti, 1766. 8vo.
(*b*) Phil. Trans., vol. xlix. p. 2, 1756.
(*c*) Phil. Trans., vol. li. p. 1; No. 28, p. 275.
(*d*) Phil. Trans., part ii. No. 45, p. 470.
(*e*) Phil. Trans., vol. li. p. 470.

where; according to Lucas, the principle impregnating all waters called sulphurous was phlogiston, which he describes as a simple elementary body, the cause of inflammability, colours, and odours in bodies, and that it is one and the same in the subjects of the three kingdoms of nature. Rutty was in some measure right, but only accidentally so, for his Synopsis is full of errors of the most glaring kind. Lucas, on the other hand, was misled by the phlogiston theory of Stahl, then in the height of its fame. In his "Cursory Examination of the Methodical Synopsis of Mineral Waters, and of the Argument of Sulphur or no Sulphur," &c.,—one of the ablest controversial pamphlets we have ever read,—as well as in his "Essay on Waters," he displays so much clear-sightedness and knowledge of the then state of chemistry, that we cannot help regarding him as one of the ablest chemists of that day in Ireland, and at the same time regretting that other pursuits led him away from a science which he might have enriched by his labours. It is unnecessary to mention the other works of Rutty, as they have been already enumerated in the notice of his life given in the Number of this Journal already referred to; but in taking leave of this estimable man, we cannot help remarking, little as the intrinsic value is which we attach to his labours, they had a powerful influence on this country in his day, and that such a man is always an invaluable member of a society, by setting an example of perseverance and industry(*a*).

We have now arrived at the period in which the Higginses lived; but before entering upon their labours it will be necessary to notice the establishment of the Royal Irish Academy, and some of the fruits of the first years of its existence, as well as the general progress in Ireland during the same period.

The activity of the Medico-Philosophical Society had diminished very much towards the end of 1780; but the spirit of research had passed beyond it, and a number of new societies were established; it is even said that a chemical club or society existed for some time. All, however, merged into the Royal Irish Academy, which was established in the very same year as Irish independence, and chartered in 1786.

The Society from which the Academy arose was formed from the union of two bodies, the one called the Paleosophers, having for its object the investigation of antiquities, and the other the Neosophers, for the cultivation of science and modern literature. It consisted of an indefinite number of members, most of them be-

(*a*) We may also mention here James Simon, although not, properly speaking, a chemist. I do not know anything of him, except that he published the following papers in the Philosophical Transactions: "Case of the Bones of a Fœtus voided per Anum," (vol. xliii. p. 529); "Account of some Fossils found in Ireland" (vol. xliii.); "On the Petrifactions of Lough Neagh" (vol. xliv. 305); "Account of the Barometer and State of the Weather at Dublin, from March 7, 1752, to February 28, 1753," (vol. xlviii. p. 305); "Journal of the Weather at Dublin, 1753, 1754, and 1755," (vol. xlix. p. 759), by far the most correct record of that time as far as it goes.

longing to the University, who at weekly meetings read essays in turn, but, "anxious to make their labours redound to the honour and advantage of their country, they formed a plan more extensive, and admitting such additional names only as might add dignity to their new institution, or by their publications had given sure ground to hope advantage from their labours, became the founders of the Royal Irish Academy"(*a*).

The first President was the Earl of Charlemont, who was succeeded in the year 1799 by Richard Kirwan. The first volume of Transactions appeared in 1787, but contained no chemical paper. Prefixed to it is a preface by the Rev. R. Burrowes, A. M., F. T. C. D., from which we cannot help making a few extracts, in order to show the spirit which animated the founders of the Academy, and also how completely the causes of our backward condition have been at all times understood, and yet how stereotyped that condition has become, and how little has as yet been done to change it. The following passage is quite as applicable to-day as it was in 1782:

"To the several advantages which Europe has within these latter centuries experienced from the cultivation of science and polite literature, this kingdom unfortunately has remained in a great measure a stranger. As no Irishman's partiality will deny this, so no man's prejudice should be suffered to make it an occasion of illiberal imputation on the capacity of Irishmen, while in the state of the country so many local peculiarities may be found fully sufficient to account for it. The important changes which took place in the Government upon the invasion by Henry II., were not carried on with so little disturbance as to permit the nation to apply itself immediately to the peaceful employments of literary inquiry; nor could it reasonably be presumed that two classes of inhabitants, entirely dissimilar in their inclinations and habits, and afterwards more widely separated by a difference in religion, should be readily prevailed on to lay aside their mutual enmity, and unite in the pursuit of speculative science. The connexion of this kingdom with England, instead of teaching Ireland the many valuable acquisitions of English industry, tended rather to entice away its men of genius to a country in which, as learning was more fashionable, its professors might be certain of enjoying more at ease the advantages of rational communication, and of receiving more ample encouragement. Its natural situation, remote from the line of immediate intercourse between any two more civilized nations, removed it also from all acquaintance with foreign improvement; and its want of political importance banished from it all those whose wealth and ambition might have introduced practical science, afforded a liberal patronage to ingenuity, and raised their country to dignity, to opulence, and to reputation.

"Such are the several causes which fatally conspired to detain this kingdom for a series of years in a state of comparative ignorance

(*a*) Preface to the first volume of the Transactions, R. I. A.

and inferiority, to counteract its natural advantages, and to derogate from its national character; and to these it is owing that, while its sons became illustrious abroad for enterprise and for ability, their country should yet have remained neglected, and its inhabitants poor, inactive, and unenlightened. But the influence of many of these causes has in a considerable degree weakened, and peculiar circumstances have now given to Ireland an importance in the political scale, which habits of well-directed industry alone can establish and maintain. Whatever, therefore, tends by the cultivation of useful arts and sciences to improve and facilitate its manufactures; whatever tends, by the elegance of polite literature, to civilize the manners and refine the tastes of its people; whatever tends to awaken a spirit of literary ambition, by keeping alive the memory of its ancient reputation for learning, cannot but prove of the greatest national advantage."

After describing the value of such societies, the origin and objects of the one just instituted, he appeals to the men of ability in the country, and points out to them their duty in supporting the Transactions of the Society by the fruits of their talents. He further tells them, "They are called on by every tie which can have a laudable influence on the heart of man, by the hopes of success and the infamy of defeat, by the solicitations of a natural instinct which will not suffer their faculties to rest without exertion, and by the authoritative voice of reason and experience which pronounce such exertions salutary, by emulation, by philanthropy, by honest pride, by a glorious sense of the dignity of their country and the dignity of human nature. To such a call Irishmen cannot be inattentive; the God of truth will look propitious on their labours, and a ray from Heaven shall light them to success." And it has done so, as is evident from the rapidly increasing importance of the Academy, though we are very far from thinking that we have done as much as we ought, but we have hopes in the future.

The first chemical paper which we find in the Transactions of the Academy is an admirable one by the Hon. George Napier, on the Manufacture of Gunpowder; it is published in vol. II. 1788.

In vol. III., 1789, we have a paper by Richard Kirwan, "Experiments on the Alkaline Substances used in Bleaching, and on the Colouring Matter of Linen-yarn." In this paper he gave an analysis of barilla, which, for the time, was admirable.

The Academy was becoming known at this time on the Continent, for we find an excellent paper in vol. III., on the Condensation produced by the mixture of alcohol and water, by M. Poujet of Montpellier, in the form of a letter to Richard Kirwan, who at that time enjoyed a considerable European reputation.

Vol. IV. contains a very long memoir of Kirwan, on the strength of acids, and the proportion of ingredients in neutral salts, into the merits of which, nor of his other memoirs, we will not at present enter, as we intend to give in a short time a notice of his labours and life. It also contains "Chemical Communications and Inqui-

ries," by Robert Perceval, M. D. This letter was intended to show that, in distilling nitric acid from nitre, the first portions which come over are heavier than the last, while in the case of muriatic acid the first and last portions are nearly alike, and the middle much less.

Vol. v. contains "Examination of the supposed igneous Origin of stony Bodies," by R. Kirwan,—read February, 1793,—being an attack on the Plutonic geologists.

"A Method of preparing a sulphurous medicinal Water," by the Rev. Edward Kenny, Rector of Moviddy, Cork: read April 6th. This water consisted in a mixture of sulphuret of magnesium, and hyposulphite of magnesia, formed by allowing an equal weight of sulphur and magnesia to act upon one another in the cold for some time.

June, 1793. "On the Solution of Lead by Lime," by R. Perceval, M. D., being an account of a great number of experiments on the action of lime-water, &c., on sheet lead, and some of the salts of lead.

June 4, 1794. "What are the Manures most advantageously applicable to the various Sorts of Soils, and what are the Causes of their beneficial Effect in each particular Instance?" by R. Kirwan. A most admirable paper, which we shall further notice at another time.

June 9, 1794. "Experiments on a new Earth, found near Strontian, in Scotland," by R. Kirwan; in which, having referred to Dr. Crawford's having made a few experiments on it in the year 1790, and to his description of it, and also the account of its chemical properties by Sulzer, he goes on to say that it appeared from his experiments to be a new earth intermediate between barytes and lime; to the presence of a carbonate of which in arragonite he conjectured was owing the difference between the mineral and calc-spar. He mentions that he was assisted in the investigation by Mr. Higgins, superintendent of the Apothecaries' Hall, whose chemical abilities are well known, and likely to be *eminently* useful to this country.

April 5, 1794. "On the Power of fixed caustic alkaline Salts to preserve the Flesh of Animals from Putrefaction. In a letter to the Rev. G. Graydon, from the Rev. Hugh Hamilton."

In vol. vi. December, 1795, we find two papers, both by R. Kirwan:—1. "Of the Composition and Proportion of Carbon in Bitumens and Mineral Coal." The method which he employed was that of fusing them with nitre. Among other specimens which he examined were Kilkenny and Leitrim coals. 2. "Thoughts on Magnetism," containing some excellent observations on crystallization, which he considered an analogous power to magnetism.

Vol. vii. contains "Additional Observations on the Proportion of real Acid in the three ancient known Mineral Acids, and on the Ingredients in various Neutral Salts, and other Compounds;" by R. Kirwan: read December, 1797.

Vol. viii. contains:—1. "A Description of an Apparatus for im-

pregnating Water and other Substances strongly with Carbonic Acid Gas," by the Rev. Gilbert Austin: read November, 1799. 2. "Analysis of Turf Ashes," by Lord Tullamore: read November 23, 1799. In this paper the author speaks of W. Higgins as his ingenious and learned friend. 3. "Observations on Calp," by the Hon. George Knox: read March 9, 1801. This paper contains rather a good analysis of calp, and appended to it is an analysis of the Lucan spa water. 4. "Observations and Experiments undertaken with a View to determine the Quantity of Sulphur contained in Sulphuric Acid, and of this latter contained in Sulphates in general," by Richard Chenevix, F. R. S.: read May 4, 1801. This is an admirable paper, in comparison with most of the others then published, although he states the quantity of sulphur in anhydrous sulphuric acid to be 61·5 instead of 40. This does not, however, detract from his merit, for Thenard gave 55·6 as the quantity, and Tromsdorff, 70.

Vol. IX., which was published in 1803, contains no chemical paper.

Vol. X. contains:—1. "A Description of an Apparatus for transferring Gases over Water or Mercury," by the Rev. C. Austin: read July 4, 1803. It is simply an account of a good mercurial trough for the sort of gaseous experiments then made. 2. "An Account of a semi-metallic Substance called Menacane, and its Ores," by G. G. Mitchell: read July 4, 1803. It is merely mineralogical, but he mentions many of the properties of titanium before the blowpipe. 3. "An experimental Inquiry into the Nature of gravelly and calculous Concretions in the human Subject, and the Effects of alkaline and acid Substances on them in and out of the Body," by Thomas Egan, M. D., Physician to Simpson's Hospital: read March 6, 1805. This is a long and valuable essay, in which he has collected all the information then known on the subject, and enriched with many experiments of his own. He does not, of course, give any chemical analysis of them, but he speaks of their solution by chemical means; and the whole is interspersed with a great many valuable remarks, both medical and chemical.

Vol. XI., published in 1810, which includes the Transactions of several years, contains but one chemical paper: "An Account of some Chalybeate Preparations in the Pharmacopœia Regis et Reginæ in Hibernia;" by Robert Perceval, M. D.: read in 1806 or 1807. In this paper he makes some very practical observations on the preparations of the acetate of iron, the double tartrate of iron and antimony, and the tinctura muriati ferri.

The period included between the foundation of the Royal Irish Academy to the year 1810, was probably the most brilliant in the history of chemistry. Bergmann, Scheele, and Margraff, had just finished their career, and by their labours created a necessity for a new chemical philosophy, which immediately arose by the labours of Wenzel, Richter, Lavoisier, Guyton Morveau, Berthollet, &c. This great progress was not without its influence on Ireland, as we have just seen by the number of papers read before the Academy,

and by a large number printed separately, or read to the Royal Society, and published in their Transactions. The most distinguished Irish chemists of this period were unquestionably Kirwan, Chenevix, and the two Higginses. Kirwan may be looked upon as the successor of Bergmann, in many of whose views he participated, both in the extent of his labours and the excellence of his character. We shall not occupy ourselves at present with his labours, as we shall do more justice to him hereafter.

Richard Chenevix, though contemporary with the Higginses, published his labours at a somewhat later period. Of his life we know little, except that he was the son of a colonel of artillery who resided in Dublin about the year 1766. His uncle was Dr. Chenevix, Bishop of Waterford, afterwards of Killaloe, the intimate friend of Lord Chesterfield, many of whose letters to him may be found in the published collection. Richard Chenevix happened to be in Paris during the reign of terror, and was thrown into prison in company with some French chemists, from whose conversation about the new chemical philosophy of Lavoisier he imbibed a taste for chemistry, which he pursued with great ardour and success on his being liberated. Besides the paper already mentioned, he published: 1. "Analyses of the Arseniates of Copper and of Iron; likewise an Analysis of the red octohedral Copper Ore of Cornwall, with Remarks on some particular Modes of Analysis"(*a*). These ores were mineralogically described by the Count de Bournon, who appears to have been an intimate friend of his. 2. "Observations and Experiments upon oxygenized and hyperoxygenized Muriatic Acid, and upon some Combinations of the Muriatic Acid in its three States"(*b*). 3. "Analysis of Corundum and some of the Substances which accompany it; with Observations on the Affinities which the Earths have been supposed to have for each other in the humid way"(*c*). 4. "Remarks upon Chemical Nomenclature, London. 1802. 8vo." 5. "Action of Platinum and Mercury upon each other"(*d*). Besides a number of smaller papers of little importance, which appeared in Tilloch's and in Nicholson's Magazines. The paper on the ores of copper is one of the best analytical papers of that time; indeed his analysis of the native red oxide is more correct than that of Proust, or of that given by Klaproth for a specimen from Siberia in 1807. In his paper on the chlorates, he fell into the errors of the time as to the nature of chlorine, nor did he succeed in isolating chloric acid. His paper on the analysis of the corundum is also very good: indeed, in analytical chemistry, there were few who excelled him. There is a circumstance connected with his paper on the "Action of Platina and Mercury upon each other," which deserves to be mentioned, as it led him to almost renounce chemistry. In 1803 an anonymous

(*a*) Phil. Trans., vol. xci. p. 193, for 1801.
(*b*) Phil. Trans., vol. xciii. part i. p. 126.
(*c*) Phil. Trans., vol. xciii. part ii. p. 327, for 1802.
(*d*) Phil. Trans., vol. xcvi. part i. p. 104, 1805.

handbill was published announcing that *palladium*, or new silver, was sold by a Mrs. Forster. This singular way of announcing a discovery led Chenevix to suppose that it was an imposition, and he consequently bought all the palladium on sale at Mrs. Forster's, and commenced its examination under the firm conviction that it was an alloy of some known metals. After considerable labour, he came to the conclusion that it was an amalgam of mercury and platina, made in a particular way,—a conclusion which was evidently the result of some mistake. The paper embodying these results was read at a meeting of the Royal Society by Dr. Wollaston, and printed in their Transactions. Soon after its appearance another anonymous handbill was circulated, offering a very high price for every grain of palladium made by Mr. Chenevix's process, or by any other whatever, but no person appeared to claim the money. In about a year after Dr. Wollaston read a paper in which he acknowledged himself to be the discoverer of palladium, and giving the method by which it was obtained from the crude ore of platinum. After this, of course, there could be no doubt that Chenevix had fallen into error, but it is impossible to account for the motive which led Wollaston to adopt this singular mode of announcing his discovery. But, whatever were his motives, his conduct towards Chenevix was anything but creditable to himself, as a philosopher or as a man, in allowing him to publish what he knew was erroneous. The result was, that Chenevix renounced, at least in a great measure, the pursuit of chemistry. In 1809, however, he showed that the same peculiar liquid was formed by the distillation of all acetates which had been already obtained from acetate of lime. To this body, the peculiarities of which he described, and which he supposed to contain less oxygen than acetic acid, he gave the name pyro-acetic spirit (acetone).

Chenevix appears also to have turned his attention to politics, for he has written several works on the questions which then agitated European Society, namely, the abstract rights of man, &c.

A few years before Chenevix published his first memoir, appeared the publications of two men who have had the misfortune to be well nigh forgotten, not only in Ireland, which is not surprising, but even among chemists in general, a fate which they by no means deserve.

The elder, Bryan Higgins, was born in some part of the county of Sligo, about the year 1737; he was educated as a physician, but I have not been able to ascertain at what college he graduated. Soon after obtaining his degree he went to London, where he practised as a physician with considerable success. He appears to have early directed his attention to chemistry, and to have carried on numerous experiments, the expense of which finally induced him as he himself says, "to open a school of practical chemistry, wherein the pupils might have uncommon advantages, at the same time that my apparatus might be enlarged, and my experiments well conducted at a common expense." He commenced his first course on

the 5th of July, 1774, in Greek-street, Soho, a syllabus of which he published in 1775. In the following year he published a part of this course under the title of "A Philosophical Essay concerning Light, vol. i., London, 1776," the intention being to publish, from time to time, the more important parts of the course. He continued to lecture in a similar manner for several years, and to have been highly patronized, among others, by the then Duke of Northumberland. In 1786 he published his best known work, which appears also to have formed the subject of part of his course, under the title of "Experiments and Observations relating to Acetous Acid, fixable Air, Dense Inflammable Air, Oils and Fuel, the Matter of Fire and Light, Metallic Reduction, Combustion, Fermentation, Putrefaction, Respiration, and other Subjects of Chemical Philosophy." 1 vol., London.

Some time between 1780 and 1790 he appears to have gone to Russia, and to have enjoyed the favour of the Empress Catherine; but I have not been able to ascertain the cause of his visit to her capital. It appears very probable, however, that he delivered several courses of lectures there. The lectures in Greek-street, thus interrupted by his sojourn in Petersburgh, were resumed in January, 1794, under nearly similar circumstances to those already mentioned; and in 1795 he published a sort of extended syllabus of his course, describing the experiments made, and the observations thereon at the discussion after each lecture, under the title of "Minutes of the Society for Philosophical Experiments and Conversations." He appears to have occupied himself a good deal with applied chemistry, and in 1789 he obtained a patent for a cheap and durable cement, composed of sand and lime, and a certain proportion of bone-ashes, the lime being slaked with lime-water instead of common water, and the mixture made use of as rapidly as possible after being made. The subject of cements was very fully treated of in a separate work published soon after, under the title of "Experiments and Observations made with the View of improving the Art of composing and applying calcareous Cements, and of preparing Quicklime. Theory of these, and Specification of the Author's cheap and durable Cement for Building, Incrustation, or Stuccoing, and artificial Stone." London, 1789.

Dr. Higgins was married to a Miss Jane Welland, daughter and heiress of J. Welland, Esq., of King's Inns, London, and he died at his estate of Walford, in Staffordshire, in the year 1820, at the age of 83. There is a very beautiful pencil sketch of him, by Comerford, which shows him to have been rather a handsome man, of an inquiring and penetrating mind, great firmness and decision of character, coupled with great affability of manner(*a*). He was rather a speculator than an experimentalist, and many of his views are, for their time, remarkable for their acuteness and generalizing charac-

(*a*) In the possession of Captain Stephens of this city, who is married to his relative.

ter. His principal works, "Experiments and Observations on Acetous Acid," &c., and his "Philosophical Essay concerning Light," have these characteristics in a most remarkable degree. I shall give a short analysis of them, in order the better to show his position as a chemist, and also to point out the influence of his views on those of his relation and pupil, William Higgins.

A large portion of the "Experiments" is occupied with an account of a series of experiments which he made on acetic acid, in which he describes its mode of preparation, purification, and properties, and the products of its distillation, and their relative proportions. In this part he describes many of the properties of acetone, a body which was already known to Lemery, Geoffroy, Rouelle, Stahl, &c., and mentions its preparation from the acetates of potash, lime, magnesia, copper, &c. Among other properties, he mentions its somniferous or anæsthetic quality, which is now known to belong to all volatile carbo-hydrogens, and to many ethers, and their chlorine and sulphur compounds. He also appears to have thoroughly understood the slow action of oxide of lead on acetic acid, in converting it into carbonic acid and water. In his chapter on the phenomena of combination, he mentions some experiments which led him to suspect the nature of the elements of nitric acid, about two years before its composition was established by Cavendish ; but, as he himself states, it did not amount to a clear knowledge of the fact. In his chapter on "Primary Notions of the Matter of Fire," he appears to have had an indistinct idea of the theory of volumes, when he enunciates a law, "that the matter of fire limits the quantity in which aeriform fluids and bodies containing it can combine chemically."—p. 307. As a consequence of this law, he states, at page 309, that "it is determined that elastic fluids shall unite with each other in limited proportions only, to form denser fluids, or solid bodies ; and that the superfluous quantities shall remain elastic and unaltered." At page 320, he seems to have considered that the atmospheres which bodies form are limited, and to have believed in the possibility of condensing most gases. "There is, undoubtedly, a natural limitation, as we formerly observed, of the forces with which the gross parts of divers elastic fluids compel and are compelled by their respective atmospheres. . . . . Although the force of chemical attraction reaches not far from the particles with any sensible effect, we are not thence to conclude that the attractive virtue ceases at any distances. But, from all the known phenomena, we may infer that the attractive forces, tending to the approximation and cohesion of gross particles, decrease in a duplicate or some higher ratio of the distances, reciprocally ; that the natural resistance to the interposition of the fiery matter is lessened at the smallest, and totally overpowered at small distances, which no eye can measure ; and, finally, that *where the sensible effect of attraction ceases, there repulsion succeeds.*" In this passage, as well as generally, he considered chemical affinity as only another form of attraction of cohesion ; and he, as Berthollet afterwards did, considered that mass had some influence on chemical affinity.

In his Essay on Light, which is a kind of Commentary on the Optics of Newton, we find some admirable ideas, for the time, on the general relations of molecules, which want of space alone prevents my analysing at full length. I shall content myself, therefore, with one or two, taken without distinction. In the chapter, "Of Gravity and Density," he draws the following distinction between these properties. "The specific gravity of a *homogeneous* body is directly as the number of ultimate parts contained in a given space. The densities of different bodies cannot be truly deduced from the specific gravity of them, unless the bodies be composed of the same kinds of matter. The absolute gravity of compounded bodies is not directly as the number of their ultimate parts, or as their quantities of matter. . . So that the absolute gravity of compounded bodies may, in the freedom of speech, be said to be in the compound ratio of the quantities of matter, and of the kinds of matter, of such bodies. The specific gravity of a compound body is in the like ratio, of the quantity and kind of matter contained in a given space. *Specific gravity* and *density* ought no longer to be used as commutable terms. Bodies whose specific gravity is comparatively small may alter the direction of the rays of light, by reason of their density, as much as is done by bodies whose specific gravity is comparatively great, by the like reason of their density."

In this passage he evidently appears to consider that most physical phenomena are influenced by the number of atoms in a given space, but that the weight of the ultimate particles of each particular substance may be different; at all events, that there is a very intimate connexion between the density of a body and its chemical constitution. In a remarkable chapter, entitled "Of Saturation," in which he sometimes appears to have some ideas similar to those afterwards established by the atomic theory, he has the following passage in connexion with this subject. Speaking of the condensation which takes place, on mixing nitrous acid fumes with air:

"And the greatest reduction of the volume of these fluids, in mixing them, is effected when they are mixed in determinate proportions; for a superabundant quantity of either fluid, added to the mixed fluids, will increase the volume of the mixture in the same manner as a portion of air added to a portion of air will increase the volume thereof."

His explanation of chemical combination consisted in attributing a mutual attraction to the ultimate particles of the same kind whence they unite in all proportion; but in bodies of a different kind, or the same body in different states, or in combination, there is a repellant power, and hence the excess of repulsive above the attractive forces is truly the cause of saturation, an idea in accordance with many of Berthollet's views.

He looked upon chemical forces as nothing more than varieties of the ordinary laws of motion; and his views on physical forces, in general, are exceedingly philosophical. He, in fact, deduces all forces from simple attraction, and a polarity which he attributes to matter. He thus announces this idea of polarity: "I am of opinion

that all simple attractions are more forcible in one direction or axis of each atom than in any other, and that all matter hath this polarity of its parts." We could quote a great number of similar passages, but we have given enough to show the tendency of his mind, and his power of seeing the direction of science in his generation. There are two very distinct classes of scientific men ; the first are the careful observers of facts, who never venture outside the domain of experiment and trifling deductions ; and the other composed of bold speculators, who feel, as it were, the future of science, and broach a number of startling theories and ideas, which only, at the end of probably fifty years, might find an architect to construct a permanent structure from, but which, in the mean time, act as a ferment to set minds in motion, and convert what was once a dream into a fact.

William Higgins was in many respects like his uncle, a speculator, and not an experimentalist, qualities which were, no doubt, increased during the time which he spent with his uncle. He also was a native of the county Sligo, from whence he was taken, while still a boy, by his uncle, and to whom he acted as assistant for one or two years previous to his entering Pembroke College, Oxford, where he graduated. After leaving the University, he returned to London, and continued to work with his uncle for some time, until a coolness arose between them, probably resulting from some discussions they appear to have had on the new chemistry then propounded in France, and of which William Higgins was one of the first and warmest supporters, while Bryan still held many of the doctrines of the supporters of the phlogiston theory. After he left his uncle he appears to have been in rather adverse circumstances for some time. In the year 1791, immediately upon the passing of their Act of Incorporation, the Apothecaries' Company of Ireland put an advertisement in the Dublin papers for a competent person to supply the place of chemist to their Hall, to which William Higgins replied, and was appointed chemist, at a salary of £200 *a year*, *apartments*, *coals*, *and candles*,—a salary which, we may remark, appears to have been considered as something *extraordinary*, as may be judged from the following passage of the letter of the Secretary to Higgins, acquainting him of his appointment : "From your character and experience, they have no doubt but you will fulfil the expectations which they entertain of you ; nor would they offer such *high* emoluments to any person whose knowledge and opportunities did not entitle him to their entire confidence"! Whether the Company thought the salary so very high, that they did not think any amount of work on the part of their chemist sufficient, or for some other reason, Higgins did not remain with them more than two or three years, when he became connected with the Dublin Society, although the salary was much less, as appears by the following minute of the Council of that body :

"Royal Dublin Society, Grafton-street ; Thursday, 18th of June, 1795. The Society purchased a collection of minerals for

£1300. Resolved,—That as Mr. Kirwan, a member of this Committee, has recommended Mr. Higgins as a proper person for the aforesaid purpose, the Committee are of opinion he should be appointed to the same ; and they submit that, from Mr. Higgins's extensive skill in chemistry, he be directed, from time to time, to make such experiments on dyeing materials, and other articles, wherein chemistry may assist the arts, as may occur ; and that, for that purpose, a small chemical apparatus should be procured, and erected at the Repository, under the direction of Mr. Higgins. Resolved,—That it is the opinion of this Committee Mr. Higgins should be rewarded for his trouble therein, in such manner as Mr. Kirwan shall direct, not exceeding the rate of £100 per an.

" Resolved,—That it is the opinion of this Society that the care of the books in their Library be committed to Mr. Higgins."

About the year 1800, a number of professors were appointed under an Act of Parliament, and William Higgins became Professor of Chemistry and mineralogy, which chair he filled until his death in 1825. During the intervening period, the only events which occurred to him, of any importance, were his being sent to London, in 1802, to assist in ascertaining the best kind of hydrometer for the Excise, and a visit of four or five months which he paid to the Continent in 1817.

William Higgins appears to have been a man of peculiar habits, possessing very great abilities, and singularly comprehensive views upon science, but totally deficient in energy, and in the ambition of working out to the end any happy idea in science which might strike him, and in that peculiar tact of putting his opinions forward in such a manner as to call immediate attention to them, without which the most important discoveries may remain for many years neglected and barren. He was incapable of making his age comprehend him. His style of lecturing was very quaint, and a number of laughable anecdotes are still remembered of circumstances the result of this quaintness, but which our space, and our respect for his memory, forbid us dwelling upon. An idea, however, may be formed of his style from his graphic illustration of chemical combination, which he described as a marriage between the ultimate particles of the combining bodies—polygamy being allowed in chemistry even beyond the Mohammedan dispensation,—for in nitric acid one atom of nitrogen marries five atoms of oxygen; and divorce was very common,—that is, chemical decomposition.

Leaving Higgins to amuse his auditory by his quaint lectures, we shall turn to the more serious duty of examining what are his real claims to a place in the history of chemistry. And before we do so we would beg to observe that we do not wish to detract from the glory of that great and excellent man, Dalton, in endeavouring to do justice to the neglected Higgins.

Higgins, if we except a "Syllabus" of his course of lectures, only published two works: the first is entitled, "A comparative View of the Phlogistic and Antiphlogistic Theories, with Inductions, &c.

London, 1789;" and a second edition, 1791; which is the work upon which his claims must entirely rest: for the second,—"Experiments and Observations on the Atomic Theory and Electrical Phenomena. Dublin, 1814,"—is simply an unworthy attack upon Dalton, and an attempt to prove his superior claims to be considered the author of the atomic theory. In order to understand clearly the question which we have to try, it is necessary to say a few words on that theory.

The laws of proportional combination, now recognised as absolutely true, are four in number. Starting from the fundamental law that *the same compound is always composed of the same ingredients*, the first of these laws, in generality, is that usually called the law of *definite or constant proportion*. According to this law, the elements composing any compound whatever are always united in the *same proportion by weight;* thus, for example, water, however formed, or under whatever circumstance it may be found, will always be composed of 1 part, by weight, of hydrogen, and 8 of oxygen; and common salt, of 36 of chlorine and 22 of the metal sodium: in this manner we can finally construct a table containing a number for each body, simple or compound, expressing the ratio in which (or in a multiple or submultiple of which) it combines with every other. This is the usual table of equivalents. That some such law as this existed was perceived very early by the Swedish chemist, Bergman, by Lavoisier and others, but its absolute truth was first fully established by Wenzel, Richter, and Proust. To this law, therefore, neither Dalton nor Higgins have any claim.

The second law, known as that of *multiple proportion*, relates to the circumstance that bodies combine in more than one proportion. It is one of the most remarkable of all, indeed it may be said to include all the others. It shows that when one body combines with another in several proportions, the higher ones are multiples of the first, or lowest. Thus, when oxygen combines with nitrogen to form laughing gas, the proportion will be found to be 14 of nitrogen and 8 of oxygen. If this body be made to combine with oxygen to form nitric oxide, the next body in the series which nitrogen and oxygen forms, it will be found that the quantity of oxygen will be 16; and if this again combine with more oxygen to form a third, the total quantity will be 24, and the fourth body will contain 32 parts, and the fifth 40 parts of oxygen; that is, the second body will have twice, the third three times, the fourth four times, and the fifth five times as much oxygen as the first. If a substance can combine with more than 8 parts of oxygen, the least next quantity it combines with is 16. It never combines with $8\frac{1}{3}$ or $8\frac{6}{7}$ths, or any other fractional quantity, but if it passes 8 it must go on to 16, and if it oversteps 16 it will go to 24, and so on for every body in nature. This law was discovered by Higgins, as we shall see immediately, from the compounds of sulphur, and by

Dalton, a number of years afterwards, from the compounds of carbon and hydrogen, and probably independently of Higgins.

The third law of combination is called the law of *reciprocal proportion*, and teaches that if two bodies combine in certain proportions with a third, they combine in exactly the same proportions with one another. Thus, if 16 parts of sulphur combine with 8 of oxygen, and 27 parts of iron combine with 8 of oxygen, 16 parts of sulphur will be found to be exactly the quantity which will unite with 27 parts of iron; and so on for all bodies. This is one of the most important laws of the whole, and is the one more than any other in relation to which the combining weights of bodies are termed their equivalents, a term which expresses that a certain weight of one body is equivalent to, or goes as far as a certain but different weight of another in the construction of a similar compound. This law was discovered by the German chemist, Wenzel, in 1777, and was enunciated in a very remarkable book which he published in that year at Dresden, under the title of *Lehre von der Verwandtschaft*. It, however, attracted very little attention until the subject was taken up by his countryman, Richter, in his *Anfangsgründe der Stöchiometrie*, 3 vols., Breslau and Hirschberg, 1792-1794; and, subsequently, in his *Neuere Gegenstände der Chemie:* Breslau, 1796-1798.

The fourth and last law is the simplest, and is readily deducible from the others; it may be called the law of *compound proportion*, and is to the effect that the combining proportion of a compound body is the sum of the combining proportions of its components. This law was first exhibited in a clear form by Dalton, but it is, in fact, a self-evident corollary to the others.

Now, from what we have said of these laws, it is quite clear that they did not originate with Dalton in any way. He was, however, the first person who saw the position of the science at the moment, and grasped at the bold views thrown out by others, and moulded them into a complete and perfect whole. The clearness and precision of his exposition of these laws was principally owing to his having adopted the old philosophical doctrine of atoms, which was held by more than one of the Grecian schools, and which is almost as fully developed in Lucretius as in any modern book. By giving this atomic theory a chemical form, and connecting the labours of Wenzel, Richter, Proust, &c., with it, he made them at once simple and popular. The laws of proportion are not Dalton's, the chemical atomic theory is indisputably his.

A strong claim is, however, set up for him, that he was the discoverer of the second law; but, as I will be able to show in the most satisfactory manner, this law was enunciated in the plainest manner by William Higgins, fifteen or sixteen years before Dalton's book appeared. The plea has been put in, that, owing to the little attention which Higgins' book excited when published, Dalton did not know of its existence. It may be so, but still it is very strange that

a book which passed through a second edition two years after the appearance of the first, and which was the earliest, and probably the best essay written in defence of the new chemistry of Lavoisier, which appeared in Great Britain, and which became very well known on the Continent a few years after its publication, should alone, of all other works on the chemical theories of the day, have escaped his notice.

The fame of Dalton does not depend on his being the author of this law. His real glory is, that what were only the first rude ideas in the minds of other men were rendered complete, and of universal applicability, by the force of his genius; there is no need, therefore, of depriving Higgins of the merit which is so justly his due. We shall now point out those passages in his book in which this discovery is clearly indicated, and also any other points in which he has stated anything new for his time.

W. Higgins has the merit of being one of the first chemists, if not the very first chemist in Great Britain or Ireland, who adopted the new views of the French chemists, in support of which he wrote the "Comparative View." He adopted these views very early, for he says, in his preface to that work, printed in 1789, that he had been an antiphlogistian for four years previously. The work is divided into ten sections:—The 1st, treating of the composition and decomposition of water. 2nd. Of the composition of acids. 3rd. Of the vitriolic acid. 4th. Of the nitrous acid. 5th. Of the marine acid. 6th. Of the calcination (oxidation) of metals, *via sicca*. 7th. Of the calcination of metals by steam; and the decomposition of water. 8th. Of the reduction of metallic calces by charcoal; and the formation of fixed air. 9th. Of the solubility of metals. 10th. Of the precipitation of metals by each other. The great question which then agitated the chemical world was, "whether or no all bodies which burn or calcine, such as sulphur, phosphorus, charcoal, oils, metals, phlogisticated air (oxygen), &c., contain the matter of light inflammable air (hydrogen), as one of their constituent principles." This question he examines with great ability and acuteness; but to the part of his book referring to this subject it is unnecessary to refer further, as the doctrine which he attacked has no longer the slightest interest for us: I shall, therefore, return to his views upon the laws of proportion.

The first idea of the law of multiple proportions occurs in the section "on the vitriolic acid." In this section he examines the doctrine of the phlogistians, that sulphur was a certain basis and phlogiston. Among the experiments which he quotes against them is the following:—"If steam be passed over fused sulphur, light inflammable air (hydrogen) will be obtained, as Dr. Priestley has observed. From whence does this air come? The phlogistians will undoubtedly say that it is disengaged from the sulphur. If so, the basis of sulphur attracts water with greater force than it does phlogiston; but will water and the basis of sulphur form vitriolic acid? By no means, though volatile vitriolic acid (sulphurous acid) is

formed in this process, which actually requires dephlogisticated air (oxygen) for its constitution, a clear proof that water must be decomposed"(*a*). After mentioning a number of objections against the phlogistian view, he goes on to say: "A good many more facts might be urged on this subject, but, in my opinion, enough has been adduced to convince an impartial reader that all the phenomena above recited are only explicable by entirely leaving out phlogiston, and supposing sulphur to be a simple substance, whose ultimate particles attract dephlogisticated air with forces inherent in themselves, independent of phlogiston or concrete inflammable air, as an alkali does an acid, or gold and tin, mercury; and likewise supposing the combustion of sulphur to be as simple a process as that of light inflammable air"(*b*).

"According to Mr. Kirwan, 100 grains of sulphur require 143 grains of dephlogisticated air to convert them into volatile vitriolic acid (sulphurous acid); but they require much more in order to become perfect vitriolic acid (sulphuric acid). Highly concentrated vitriolic acid contains two parts of dephlogisticated air, and one of sulphur, exclusive of water. . . . We may conclude that the ultimate particles of sulphur and dephlogisticated air contain equal quantities of solid matter; for dephlogisticated air suffers no inconsiderable contraction by uniting to sulphur in the proportion merely necessary for the formation of volatile vitriolic acid. Hence, we may conclude, that in volatile vitriolic acid a single ultimate particle of sulphur is intimately united only to a single particle of dephlogisticated air; and that in perfect vitriolic acid every single particle of sulphur is united to two of dephlogisticated air, being the quantity necessary to saturation"(*c*).

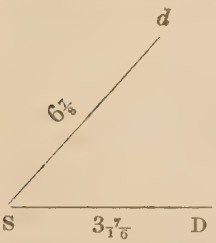
"As 2 cubic inches of light inflammable air (hydrogen) require but one of dephlogisticated air (oxygen) to condense them, we must suppose that they contain equal number of divisions, and that the difference of their specific gravity depends chiefly on the size of their ultimate particles; or we must suppose that the ultimate particles of light inflammable air require two or three, or more of dephlogisticated air to saturate them. If this latter were the case, we might produce water in an intermediate state, as well as the vitriolic or the nitrous acid (nitric acid), which appears to be impossible; for, in whatever proportion we mix our air, or under whatever circumstance we combine them, the result is invariably the same. This likewise may be observed with respect to the decomposition of water. Hence, we may justly conclude that water is composed of molecules, formed by the union of a single particle of dephlogisticated air to an ultimate particle of light inflammable air, and that they are incapable of uniting to a third particle of either of their constituent principles"(*d*).

In these extracts we have not only the second law of proportion clearly indicated, but his idea of the composition of water is all but

(*a*) Comp. View, p. 33. Second Ed. (*b*) Ibid. p. 35.
(*c*) Ibid. p. 36. (*d*) Ibid. p. 37.

perfect; and he has almost enunciated the volume theory of Gay-Lussac in the last passage, where he assumes that equal volumes of bodies contain the same number of divisions, that is, of atoms. He did not give the composition of sulphurous and sulphuric acid correctly, simply because Kirwan's numbers, upon which all his calculations are founded, differ very considerably from the truth; but whether sulphuric acid consists of two atoms of oxygen, or of three atoms, does not in the slightest manner affect his views.

But not only did he apply these views to the compounds of sulphur and oxygen, and to water, but also to the compounds of nitrogen and oxygen, the composition of nitric acid being very accurately given by him in the following passage:—"In my opinion the purest nitrous acid (nitric acid) contains five of dephlogisticated air (oxygen), and one of phlogisticated air (azote); nitrous gas, according to Kirwan, contains two volumes of oxygen gas, and one of azotic gas; according to Lavoisier, 100 grains of nitrous gas contain 32 grains of phlogisticated air, and 68 of dephlogisticated air. I am of the former philosopher's opinion. I likewise am of opinion that every primary particle of phlogisticated air is united to two of dephlogisticated, and that these molecules are surrounded with one common atmosphere of fire (caloric)." Throughout the "Comparative View" he uses diagrams representing triangles, &c., for expressing the affinity of the different molecules for one another; and over each line is placed a number expressing the relative force of affinity. Thus, where S is a particle of sulphur; *d*, a particle of dephlogisticated air, which it attracts with a force of $6\frac{7}{8}$, and the compound of these two sulphurous acid: as the attraction of bodies is mutual, S is supposed to possess one-half of this force, that is $3\frac{7}{16}$, which is supposed to be its utmost exertion, the other half of the $6\frac{7}{8}$ being supposed to be exerted by the particle *d*, causing them to unite. Now, if we suppose another particle of dephlogisticated air (ogygen), D, to have a tendency to unite to S, with the force also of $3\frac{7}{16}$, in order to form perfect vitriolic acid (sulphuric); to receive D, S must relax its attraction for *d* one-half; that is, the force of $3\frac{7}{16}$ will be divided and directed in two different points, which will reduce the attachment of dephlogisticated air and sulphur in sulphuric acid to $5\frac{1}{16}$: thus, according to him, accounting for the greater facility with which the second atom of oxygen is removed from such compounds. This mode, clumsy as it is, was exceedingly original, and expressed something more than the symbols of Dalton, but included more than the state of chemistry at that time, or even at present, would warrant.



It may be necessary to remark here, that Higgins applied the term "ultimate particle" to the last division of elementary matter; and the term "molecule," to the last division of a chemical com-

pound. The first corresponds to an atom of a simple substance (adopting the atomic theory); and the second, to a compound atom.

Higgins' notions of chemistry were extremely mathematical; in fact, from many passages throughout his book it would appear that he considered that chemical affinity should be considered from a purely mechanical point of view. The following passage, in which he takes motion, or rather the momentum, of the ultimate atoms into account, is certainly very singular:—"Therefore," speaking of the nature of the action of sulphuric acid on iron, "it is the superior force of the metal, and the *proportional velocity* of the motion of oxygen towards it, that leave the particles of sulphur so circumstanced as to enable them to decompose water in the manner already described." A great number of passages of a similar nature, full of ingenious ideas, might be given, which show that he possessed, in a very high degree, an imaginative and philosophical mind; but, as I have already passed the limits assigned to such essays as the present, I must refer the reader to the original work, which is very well worth an attentive perusal.

That I have not overstated the claims of William Higgins to be considered the discoverer of the second law of proportions will be seen from the following statements with reference to this point, made by Sir Humphrey Davy, Dr. Thomson, and Dr. Daubeny. Davy thus speaks of the matter: "That the proportion in compound gases are definite have long been generally acknowledged; but Mr. Higgins is, I believe, the first person who conceived that, when gases combined in more than one proportion, all the proportions of the same elements were equal; and he founded this idea, which was made public in 1789, on the corpuscular hypothesis that bodies combine particle with particle, or one with two or three, or a greater number of particles." Mr. Dalton, about 1802, adopting a similar hypothesis, apparently without the knowledge of what Mr. Higgins had written, extended his views to compounds in general(*a*). Dr. Thomson makes the following remark, the first line being, however, a simple absurdity: "The atomic theory was taught by Bergman, Cullen, Black, &c." (the two latter were supposed to be Scotchmen, and of course nothing could be discovered that was not in some way anticipated by men of that country), "just as far as it was by Higgins. The latter indeed states some striking facts respecting the gases, and anticipated Gay-Lussac's theory of volumes . . . ;" (the latter, it must be remembered, was only a Frenchman) (*b*). Dr. Daubeny, is however, more just than Thomson, his account being as follows:

"It would appear that we owe to Mr. Higgins, formerly of Pembroke College in this University (Oxford), the first enunciation of this latter fact (the law of multiple proportion); as in a work published by him in 1789, entitled, 'A Comparative View of

(*a*) Elements of Chemical Philosophy, p 107.
(*b*) Annals of Philosophy, December, 1813.

the Phlogistic and Antiphlogistic Theories,' he distinctly states that one ultimate particle of sulphur, and one of oxygen, constitute sulphurous acid, whilst one ultimate particle of sulphur and two of oxygen constitute sulphuric acid; and moreover, that in the compounds of azote and oxygen, the ingredients are to each other in the proportion of 1 to 1, 2, 3, 4, 5, respectively.

"He also throughout his work adheres to the corpuscular hypothesis, supposing matter to combine particle to particle, though in this latter respect indeed he only followed the notions of former chemists, who, if they thought at all upon the subject, generally leaned to the opinion that chemical combination took place between the ultimate molecules of matter, and not, according to the Kantian doctrine, owing to a mutual penetration of one substance by another.

"It is a pity for Mr. Higgins' reputation that he had not struck out this idea a *few years later*, when, as its correctness could have been substantiated by an appeal to facts, it is probable that his genius might have overleaped the boundary that separates the point to which we are led by his researches from that afterwards attained by Mr. Dalton.

"As it was, the want of precision in chemical analysis rendered it impossible at that time to collect a number of instances sufficient to establish the law as holding good universally; . . ."(*a*)

These passages fully justify the view I have taken of the labours of Higgins, especially the last, which puts the matter in the clearest point of view. I could quote a number of similar opinions, but I believe those which I have given sufficient.

Hasty and imperfect as this sketch is, I hope I have succeeded in rescuing some deserving Irishmen from oblivion; and that I have not, on the one hand, given too much importance to some, or unjustly detracted from the merits of others. In conclusion, I would express a hope that the other branches of science may find some persons to rescue from neglect the memories of their humble, but deserving, Irish votaries(*b*).

(*a*) Daubeny's Introduction to the Atomic Theory, pp. 33, 34.

(*b*) I am indebted for most of the details of the life of Bryan Higgins to Captain and Mrs. Stephens, of Holles-street (the latter the niece of Dr. Higgins), who in the kindest manner supplied copies of all his works, and of those of William Higgins, and also the beautiful pencil sketch of Bryan Higgins mentioned in the text. I beg also to return my best thanks to my friend Dr. Leet, of Stephen's-green, for the information which he communicated to me relative to William Higgins' connexion with Apothecaries' Hall.

MEMOIR OF THE LATE THOMAS LITTLE, M.D., LL.D., T.C.D., F.R.C.S.,

SURGEON TO THE SLIGO COUNTY INFIRMARY.

This eminent surgeon and physician was the eldest son of William Little, Esq., formerly Deputy Examiner in the Irish Court of Chancery, which office he held under his brother-in-law, Lyndon Evelyn, M.P. for St. Ives. Upon the abolition of this office, Mr. Little was appointed to the secretaryship of the Paving Board, a Government situation under the old Corporation of this city, which post he held for a great number of years, until superannuated in 1828, at the age of 77. The subject of this memoir was born in Jervis-street, Dublin, on the 11th November, in the year 1782, and, consequently, at the period of his death by cholera on the 14th of August, 1849, was in his sixty-seventh year.

In 1799 he matriculated in Trinity College, Dublin, and in the following year was bound apprentice to Mr. Mac Evoy, the celebrated surgeon. This able and eccentric man, the Irish Abernethy of his day, notwithstanding the wide gulf which, in those times, separated the master and the pupil, the professor and the student, and in his case the more particularly, owing to a natural harshness and even coarseness of manner, and quickness of temper, which seldom observed the refinements of life in his hospital and lecture-room intercourse with his pupils, early elicited in his young disciple the warmest admiration and most profound reverence for his genius and abilities; and to the last, Mac Evoy's maxims, whether delivered in lecture, or dropped in casual conversations, were treasured up with astonishing fidelity, in a memory that seldom allowed a professional fact or a scientific hint to escape; and, on the other hand, it is well known with what eccentric affection, and by what awkward demonstrations of good-will, old Mac Evoy honoured his young friend.

In the year 1803, Mr. Little, then in his twenty-first year, became a prominent and popular member of the College corps, in which he was soon raised from a private to be corporal, a sergeant, and finally to that important post of *fugleman*, and afterwards contested the lieutenancy, but whether successfully or not the writer's recollections do not enable him to record. At this period he was remarkable for feats of activity, and his performance of the very difficult functions of fugleman at reviews in the Park frequently elicited the admiration of the veterans of that day, in whose eyes this now obsolete and absurd department of military tactics possessed a peculiar and most vivid charm. It was about this period, when performing one of his favourite feats of vaulting over an open room door, as it swung under his hands, that he received a frightful compound fracture of the right tibia and fibula, a few inches below the knee, which at one time promised so badly that Mr. Mac Evoy had brought his instruments and assistants into the sufferer's bed-room,

to perform amputation; but, having fortunately discerned some contra-indication to this *dernier resort*, its consideration was deferred, and the result was a perfect cure.

It was at this period also that Dr. Little formed many valuable acquaintances with men who have since distinguished themselves in law, literature, science, the Church and the State; and much of the zest of his well-known and incomparable conversational powers was derived from anecdotes of the morning drill and midnight watch of these troublous times, in company with the Cramptons, Blackburnes, and Norths, of past and present days, on which occasion a well-furnished canteen of shrub punch formed quite as necessary an item of the academico-military equipment as the firelock or cartouch-box, and under the influence of which upon young, ardent, and excitable minds, it is not difficult to imagine that many and many a manœuvre was accomplished, and many a *chef d'œuvre* of strategy effected, more suited to enliven a page of Lever or Maxwell than to establish the military reputation of the academic corps in the stern eye of a Wellington or a Napier.

In the year 1804, by the interest of Mr. Obrey, then Surgeon-General, and an old friend of his father's, Thomas Little having expressed a decided predilection for a military life, an inclination probably fostered by the recent memory of the glories of his fugleman-days in the College corps, he was appointed on the medical staff at Cork, where he renewed his acquaintance and friendship with his old school-fellow, the lamented Richard Carmichael. Through the subsequent rather long lives of both this friendship was matured by frequent meetings and constant correspondence, and by a mutual appreciation of each other's characters and talents; within a few months both were removed, the one by a fearful accident, the shock of which was felt from one end of the country to the other, and the survivor almost as suddenly by an attack of cholera in its most acute and deadly form of invasion.

While stationed on the Staff in Cork, an attachment, which subsequently resulted in his marriage, and which would go far to establish the popular fancy of the destiny which attaches to this condition, was formed between Dr. Little and Miss Swayne, daughter of Benjamin Swayne, Esq., a gentleman of independent fortune in the neighbourhood of that city. The youth of the parties, and other considerations, rendering the match not a desirable one to the old people, all interest was made to remove the gentleman, who was at that time considered one of the handsomest and most agreeable men in Cork, the result of which was his immediate appointment to the assistant-surgeoncy of the 23rd Dragoons, then named for the Peninsula. He joined this corps at Chatham, but, by some change of design at the War Office, in a few weeks they were ordered to Fermoy, within eighteen miles of Cork, where the intercourse was immediately renewed, as a matter of course; all parties yielding with a good grace to what now might be considered a special manifestation of *destiny* in its favour.

When the 23rd were in reality ordered, in about eighteen months afterwards to the Peninsula (where, by the way, our readers will recollect they were shortly afterwards nearly annihilated at the battle of Talavera), Dr. Little (now a married man) effected an exchange into the North Mayo Militia, as surgeon, which commission he purchased, and subsequently sold for £500, as was the fashion of the times. This step it was which laid the foundation of his long and successful connexion with the province of Connaught, and of his undisputed reputation as the most distinguished practitioner and operator who had ever pitched his tent west of the Shannon.

During the eight years that Dr. Little was surgeon of the North Mayo Militia he had rapidly acquired a character as an operator of extraordinary merit, and a physician of great success and skill, so much so that the poor of the various localities where the regiment was quartered, in the provinces of Leinster, Munster, and Connaught, attracted by the reputation which preceded and accompanied him, were accustomed to flock in hundreds to his military hospital, where that unbounded zeal in his profession, that untiring activity of mind and body, and that universal philanthropy, which he retained to his last day of life, and to which, in fact, that valuable life was ultimately sacrificed, led him to toil unceasingly in unpaid, but not unrequited services; for the tenants of the neighbouring gentry, carried back to their landlords and friends the warmest, and sometimes exaggerated eulogiums on his skill and attention, so that he invariably and at once occupied the leading practice of the entire neighbourhood. When the Dispensary Act appeared (in 1807) he immediately endeavoured to impress upon the gentry of the localities where the regiment then happened to be quartered, the immense value of its provisions for the sick poor, and succeeded in establishing, *en route*, three dispensaries, one in Rathkeale, county Limerick, one in Arklow, county of Wicklow, and a third in Tuam, county of Galway.

In the year 1814 the North Mayo Militia, then lying at Tuam, were disbanded, and Dr. Little, having resolved on adventuring metropolitan practice, took a house on Ormond-quay; but during his sojourn in Tuam, then a flourishing town (very different indeed from the indigent and deserted aspect it now presents), and the centre of a thickly populated and even wealthy country, practice flowed so thick and fast upon him, and the solicitations of his brother officers, almost all men of wealth and influence in the adjoining county of Mayo, were so urgent that he should remain where he was, that, after some reflection, though with much regret, he disposed of his house in Dublin, and soon afterwards, at the early age of 34, found himself in the receipt of nearly £1500 a-year, the proceeds of his professional labours, perhaps double the annual sum ever derived from medical practice in the province of Connaught before.

Under the patronage of Lord Decies, then Archbishop of Tuam, a prelate of unbounded benevolence and princely hospitality, the

first dispensary ever established in the county of Galway was opened in the year 1814, by the suggestion and under the management of Dr. Little, with a small hospital adjoining, of eight beds, for operative surgical cases; and such were the circumstances of the country, or the estimation in which its services were held, that for many years its average income was about £400 a-year. To this miniature hospital, some twenty-five years since, crowds of the halt, and maimed, and blind, from every county in Connaught, and from many of Munster, Leinster, and the North, were constantly flocking; and in a small cabin of an insignificant country town in the far west, the curious in surgery would be surprised to find a rapid succession and unceasing variety of the most formidable cases, either awaiting or in the different stages of convalescence after operation. To the present day there are hundreds of the aged poor about the counties of Limerick, Clare, Mayo, Galway, Sligo, and Roscommon, who will tell, with the greatest gravity, and with a downright belief in what they have repeated so often as to make the tale pass current even with themselves, that in such a year, and at such a place, Dr. Little removed such a man's eye, washed it, and restored it perfectly sound to its orbit, or performed similar excisions and ablutions with the liver, bowels, and brains of others. Nor do we instance these popular exaggerations merely in order to dwell upon their absurdity, or excite a smile of compassion for human credulity, but because we consider the existence of such as the best possible proof of the extraordinary boldness and success of Dr. Little as an operator.

It is much to be regretted that, with a mind stored with all that was valuable in either ancient or modern medical literature, together with a vast amount of miscellaneous information on almost every subject (for to the last day of his life, and during the busiest periods of his practice, he was an industrious reader and student), with the most acute powers of observation, an exceedingly correct and cautious judgment, a wonderfully retentive memory, which enabled him, when conversing on medical subjects, to recall the minutest details of relevant cases which had interested him twenty or thirty years before, and to illustrate his positions with a torrent of quotations, frequently in the very words of the authors,—possessing a minute acquaintance with, and an experimental knowledge of almost every modern suggestion or established improvement in each department of his profession;—it is much to be regretted, we repeat, that, with all these advantages, together with a great facility of expressing his ideas simply and clearly (as many letters in the Editor's possession abundantly prove), he should never have contributed to the literature of that profession which he so ardently loved and successfully cultivated. The secret of his backwardness to commit himself to the irrevocable step of publication can only be explained on the ground of a morbid sensitiveness to public opinion, a weakness or a virtue as the reader may deem fit to term it, which was a remarkable feature of his character, and an apprehension that his opportunities for cultivating a minute acquaintance with morbid anatomy

had been much too limited (as, in the most extensive *country* practice, they necessarily must be) to enable him to lay his views before the profession in that perfect and comprehensive style which so elicited his admiration in the writings of others of the modern pathological school of authors; and anything less than the perfection he aimed at would not have satisfied him.

As an operator it was scarcely possible that he could be excelled; and in the country he performed every form and variety of operation, amputation at hip-joint, of lower maxilla, lithotomy, hernia, ligature of all those vessels ordinarily submitted to this operation. He possessed great nerve and celerity, with the most astonishing aptitude and readiness in adopting, without a moment's hesitation or confusion, exactly the proper series of manipulations which more or less vary the history of every great operation.

In the year 1832, Dr. Little, having already refused the proffered appointments to the Wicklow and Longford County Infirmaries, and while in the zenith of his reputation and practice, was nominated, without the preliminary steps of solicitation or canvass, to the surgeoncy of the Sligo County Infirmary, vacant by the death of Mr. Bell, by cholera. The highly complimentary manner of this offer, and the fact that this change of residence, while it introduced him to a new and most influential circle of practice, would not materially remove him from his lucrative connexion with the county of Mayo, at once determined him on accepting this highly honourable position. An illegal election having been held in the first instance, Mr. Cooper, of Markree Castle, instituted proceedings in the King's Bench, which quashed the appointment then made, and Dr. Little was on the subsequent election appointed by a large majority. The best evidence of the estimation in which he was held during the last sixteen years of his life, will be found in the notices of his lamented death by the local Sligo papers of diametrically opposed political principles(*a*), and in the all but unanimous appointment of his eldest son, Dr. W. S. Little, to the surgeoncy of the excellent institution so long and effectually presided over by himself.

(*a*) See the Sligo Journal of August 17th, and Champion of August 18th, 1849.

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